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CONTENTS.

PREMIUMS FOR 1863.

At the Meeting of the Society, on January 18th, 1864, Premiums of Books were presented to:—

ZERAH COLBURN, for his Paper "On the Ultimate Strength of Iron."

And to

GEORGE GORDON PAGE, for his Paper "On the Construction of Chelsea Bridge."

January 12*th*, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON "STEAM-BOILER EXPLOSIONS."

BY PERRY F. NURSEY.

FROM whatever point of view the subject of "Steam Boiler Explosions" is considered—whether in the office, the works, or the study—a seriousness and earnestness of purpose must ever accompany either practical directions or theoretical deductions professing to ameliorate the evil. An amount of responsibility attaches to the expression of an opinion, either actively or passively, which may influence ultimate results, even to life or death. It often turns upon a very little point whether one of the greatest instruments of peace and prosperity shall not produce results far more fatally disastrous than those issuing from engines of war. The history of steam engineering is accompanied, step by step, by a corresponding history of disaster; and in the present day, notwithstanding the many preventive measures adopted, it is lamentable to find that, on the whole, no greater immunity in respect of boiler accidents is insured than was in years long gone by—indeed, it would almost appear to be far less. Although much has been done, and is still doing, to diminish the probabilities of accident, the best exertions can command but partial success while the primal causes remain shrouded in mystery, for it is notorious that cases are constantly occurring for which the most searching investigation fails to establish an adequate cause. Many theories as to the cause of the violence exhibited have from time to time been called into existence by the characteristics attending various steam-boiler explosions; some of these find support in circumstances which are fatal to others, and *vice versâ*, whilst most of them are open to entire objection upon the score of being inconsistent with certain fixed laws of cause and effect.

The records of every passing year tell, with sad persistency, the tale of death upon death, and the destruction of valuable property.

During the past twelve months these accidents have been especially numerous and fatal. At a cursory glance there are to be found on record more than thirty boiler explosions, each attended with loss of life—in one case six, and in another fatally disastrous one no less than twenty-eight lives were sacrificed. In the previous year, 1861, twenty explosions were known to have happened in various parts of the kingdom, whereby twenty-seven persons were killed and nearly fifty wounded.

It will be as well to notice the various causes from which accidents generally arise. Unequal expansion in different parts of the boiler gives rise to the fracture of plates and angle irons. The bottoms of double-flued boilers internally fired are specially subject to these fractures, which takes place in the transverse or ring seams in the middle of their length, and are more severe in long than in short boilers, especially when the feed is admitted cold and at the bottom. In most cases fractures in the angle irons occur at the upper part of the ring at the furnace crown, and result from the constant buckling action induced by the alternate expansion and contraction in the upper part of the furnace.

There are many instances of internal corrosion, caused by the action of water impregnated with acid. In some cases the whole plate is honeycombed, in others this appearance is confined to small spots and some of the rivet heads. Badly made boilers suffer severely at the seams from the use of corrosive water. External corrosion is attributable to external damp, but it may be caused by water penetrating into the flues and saturating the seating, or from blowing seams and rivets. Those injurious effects are much accelerated when the boiler is in contact with brickwork, which retains the water and holds it against the plates; and oxidation rapidly reduces the thickness of the plates.

Channelling has been attributed to various causes. Cases frequently occur near to the ring seams at the bottom of double-flued boilers, internally fired, under similar circumstances to fractures already noticed. The similarity of their position has led to the supposition that they are the result of oxidation caused by a slight leak at the joint. This may frequently be the case, but instances have been met with where no external damp could be discovered, neither could any

trace of leak be detected. This channelling may be due to disintegration of the metal caused by the constant buckling action in plates in immediate proximity to seams of rivets when submitted to heavy strains. Channelling has also been attributed to the chemical action of the gases consequent on the nature of the fuel used; but it is admitted that the question is one which requires further minute observation, as well as careful consideration of facts recorded.

Safety valves of defective construction are not unfrequent; in some the dangerous practice of passing the spindle through a stuffing-box is still retained, in others the point at which the lever presses on them is placed too high above the face, so that they do not readily seat after blowing; a very bad system is that of *irregularly* loading the safety valve with any pieces of scrap iron or chain that may serve as weights.

The safe working of a boiler depends further upon the fittings for blowing out, the taps and valves for which demand careful attention as to their proper action. Slides or sluices work freely, but are sometimes uncertain in their closing; the lodgment of sediment in the bottom of the boxes will render it impossible to close them. The feed apparatus and glass water gauges (of which, for safety, there should be two to each boiler), also require special care. With regard to the former, Mr. L. E. Fletcher (the chief engineer to the Manchester Boiler Association), observed in his annual report for 1861, that there was a description of feed-stop valve which he found in very general use of an inconvenient and even dangerous character; and he calls attention to the fact, with a view to discontinuing its use in new boilers and correcting it in existing ones. The valve he refers to is constructed on the suspension principle, and is opened against the force of the feed by a spindle attached to it, so that, should the valve at any time break away, it drops to its seat and cuts off the supply. He found several cases where the connexion with the spindle had given way unawares, in some instances causing the stoppage of the works, and, in one case explosion, and loss of life. The remedy he suggests is simple and inexpensive—to dispense with the suspension feed-stop valve altogether, and regulate the supply by controlling the lift of the back pressure valve by a screwed spindle placed above it, but entirely disconnected from it. Mr. Fletcher

notices also a gauge of a novel description, which admits of the interior of the boiler being seen when in actual work; this is accomplished by a glass eye-piece, the boiler being illuminated by means of a light shed through a lens which lights up the whole interior of the boiler and shows the surface of the water in ebullition.

Incrustation may prove a frequent cause of accident, if due precautions to prevent it are not taken. Much inconvenience arises in some districts in this respect, owing to the nature of the water employed; but the evil may be much lessened by proper attention to blowing out, which is far preferable to the use of boiler compositions where the operation is not confined to one point only at the bottom of the boiler.

With regard to the accidents before noticed, which occurred during the year 1861, Mr. Fletcher observes in his able report, already quoted, that from personal examination of several of the boilers after the accidents, and careful consideration of the facts in the other cases, he found that due care and periodical inspection, with the application, where necessary, of the hydraulic test, would have prevented every one of the explosions, and thus that the word "accident" could not be applied to any one of them. No explosion had happened to any boiler under the inspection of the Association during the whole year (1861), although at its close there were under inspection, at 535 factories and other works, 1454 boilers of every variety in mechanical construction. The value and importance of this supervision cannot be overrated when it is considered how many accidents arise from the want of an occasional careful examination, which it is not always possible to expect from the attendants in charge, who, in many instances, have shown a carelessness highly criminal. Witness the recent explosion at Rising Bridge, by which three lives were lost; here the cause of the explosion was clearly corrosion of the plates, which in one part were found to be as thin as a sixpence. No one could have swept the flues without discovering the corrosion, which had been going on for two years. Had the engine-tenter done his duty, he must have detected it.

The locomotive boiler explosion that occurred on the 4th of July, 1861, near Rugby, on the London and North-Western Railway, is another of many instances illustrating the necessity of an occasional

careful examination. In this case the barrel of the boiler was blown to pieces while the fire-box remained almost uninjured. The engine was constructed by Sharp Brothers and Co., and had been working about ten years. It had 142 square feet of heating surface in the fire-box, and 1152 square feet in the tubes, of which there were 195. It was a six-wheeled single engine, with 16 in. cylinders, 22 in. stroke, and 7 ft. driving wheels, was provided with two safety valves $3\frac{5}{8}$ in. diameter, set to a pressure of 100 lb. on the square inch, and a Bourdon's pressure gauge. It ran 196,885 miles between October, 1851, and October, 1857, when it was supplied with a new set of tubes, and ran 139,485 miles between that date and the period of explosion. The barrel of the boiler was composed of $\frac{3}{8}$ in. plate, and the outer shell of the fire-box $\frac{7}{16}$ in. plate. On examination, after the accident, Captain Tyler found that corrosion had been actively going on in several parts, particularly along two seams of rivets, one on the middle ring and the other on the fire-box ring: in some places the metal had been reduced to about 1-16 in. in thickness, at which rupture might be expected at the ordinary working pressure of the engine =120 lb. The corrosive action which clearly led to the explosion must have set in before the re-tubing in October, 1857, but was probably considered undeserving of notice at that time. Captain Tyler observes that it is more probable that the reduction to about 1-6th of its original thickness, which was apparent in the plate that failed, had been spread over the nine years and eight months that the engine had been at work, and that the plate must have been in a condition to require renewal when the new tubes were put in. If not, then the plate had been reduced in three years and eight months from its original thickness, or nearly, to its then condition; clearly proving the necessity of more frequent inspection. The factor of safety originally allowed in that boiler was only $4\frac{1}{2}$ instead of 6, which, in Captain Tyler's opinion, it ought to have been, inasmuch as the working pressure was 120 lb. and the bursting pressure 520 lb. per square inch; and the full working pressure was maintained while the plates were getting thinner and thinner, until at length the boiler exploded in the ordinary peformance of its duty.

It will be a matter of somewhat anxious interest to ascertain the number of boilers of from 9 ft. to 10 ft. 6 in. diameter now at work

in the mining and manufacturing districts. Made originally of $\frac{3}{8}$ in. or $\frac{7}{16}$ in. iron, they are worked, after fifteen or twenty years' use, at pressures of 35 lb. and 40 lb. per square inch, seldom receiving thorough inspection by competent men, and generally attended by ignorant and poorly-paid enginemen. About the middle of April last year one of these huge vessels exploded at Priestfield without warning, and twenty-eight men were killed and ten more severely injured. The boiler was 10 ft. 6 in. diameter and about 30 ft. high, made eight or nine years ago of $\frac{1}{2}$ in. iron, and was considered safe at 50 lb. pressure, being often worked at 40 lb. A few weeks previously a boiler 9 ft. diameter, 21 ft. 3 in. high, made of $\frac{7}{16}$ in. plates, and worked eight years under a pressure of 50 lb., blew up, killing six men. In that case there was little doubt as to the cause of the explosion, for there was a vertical internal flue no less than 4 ft. 8 in. diameter, the nearly flat head of which was not stayed at all. A few years ago a boiler 36 ft. 6 in. long, 9 ft. 1 in. diameter, made of $\frac{3}{8}$ in. plates, and which had been worked for eleven years at a pressure of 40 lb. per square inch, blew up with fearful violence; and a little later a boiler 24 ft. long and 10 ft. diameter, made eighteen years before of $\frac{3}{8}$ in. plates, exploded at Tipton.

In these explosions, however, there is little, if anything mysterious. Out of all proportion in size, made of ordinary and sometimes inferior iron, old, badly tended, and, in the mining districts, often exposed to the insidious action of corrosive water, what is to be expected in these huge bombshells but ultimate explosion?

Returning to more recent catastrophes, note the fearful explosion at High Moor Farm, near Alnwick, which resulted in seven deaths and as many severe injuries. The boiler appears to have been a very old one, and was carried completely through the roof of the building, which was knocked into ruins. The boiler was hurled to a height of forty feet over a straw barn, falling in the yard on the other side. Then the explosion at the Midland Ironworks, Masborough, by which a number of men were killed. Several boilers of various dimensions were fixed in the rolling mill, one of these—nearly the largest—was embedded between two smaller ones about eighty yards from the entrance gates. They were under a shed covered with sheet-iron and slate, and supported by wooden and iron beams crossed

upon iron pillars. Work was proceeding as usual, when, with a tremendous report, the boiler launched itself forward into the mill, and in an instant the whole place was in ruins. The two large boiler tubes were forced to the rear of the premises. The supports of the roof were broken, solid iron columns 12 in. or 14 in. thick, snapping short off. The collapsed tubes and numerous rents in the boiler indicated a want of the proper supply of water.

A Great Western passenger engine blew up while steam was being raised on it in the Paddington shed, on the morning of 8th November last. A North-Western goods-engine exploded on the 5th of May previous, and a North-Eastern engine met a like fate at Stella Gill in October, 1861.

Each of these three last cases could be readily traced to similar causes to those mentioned in the case of the Rugby explosion, namely, corrosion of the plates—and it is by no means unusual for locomotive boilers to fail in this way. It is a noticeable fact that these explosions are common only to certain lines of railways, while others are exempt from them. The question, how this is to be accounted for, very naturally suggests itself, and finds a solution in the fact that, on the Eastern Counties, South-Eastern, Lancashire and Yorkshire, Caledonian, Edinburgh and Glasgow, and some other lines on which boiler explosions are unknown, the hydraulic test has, for some time, been employed for ascertaining the soundness of boilers; hence it happens that boilers do not blow up on these lines; whereas, on the Great Western, London and North-Western, Great Northern, Midland, North-Eastern, and some other great lines, where this test is not adopted, such disasters are becoming somewhat common. The objection raised by experienced locomotive engineers to the hydraulic test is that it is likely to injure the boiler. Injury would, doubtless, result if a boiler were once strained beyond the limit of elasticity of the iron. But a boiler, the bursting pressure of which was, as it ought to be, 600 lb. per square inch, could not be injured by a temporary water pressure of 250 lb. At the same time, to ascertain the actual strength of a boiler, it must be burst open experimentally; and, therefore, the boiler, supposed to be capable of bearing 600 lb. pressure before giving way might really have a strength only equal to 300 lb., in which case a test pressure of

250 lb., although it might not actually rupture the boiler, would undoubtedly injure it, and thus hasten the explosion, which, even without the test, would have been likely, sooner or later, to have taken place. Notwithstanding, therefore, that the hydraulic test is extensively and successfully used, there may be reason, in some cases, for hesitation in adopting it. And yet, again, there need be no grounds for apprehension on this point, for—to follow the sound arguments and practical suggestions advanced by the able writer of a recent article in *The Engineer*—it is well known that iron is only injured by injuring its elasticity, and its elasticity is injured only when it shows, after being strained, permanent elongation of the fibres, and this may be produced by a strain somewhere between one-third and one-half its breaking strength. Thus, a boiler, the bursting strength of which was 800 lb., would be found to have had its diameter slightly but permanently increased under a strain of 400 lb.; under a strain of 250 lb. the boiler would remain safe under a steam pressure of 150 lb. per square inch. But why not adopt the practice of measuring the diameter of boilers under successive increments of strain? Take the exact diameter of the largest ring of a boiler charged with cold water to a pressure of 100 lb.; measure it again, with the pressure increased successively to 150 lb., 200 lb., 250 lb., and so on. Then ease off the pressure, and note to what extent the iron has been permanently stretched. If at 100 lb. pressure, or when free from strain, the diameter is exactly what it was before the test, the boiler is safe with any ordinary load on the safety valves. If permanently enlarged to any considerable extent, the boiler needs immediate strengthening. Iron bridges, before being opened for traffic, are loaded with the greatest weight that can ever come upon them, the deflection of the arches or girders is carefully noted, the amount of permanent set—if any—ascertained, and their safety is inferred not so much from the fact that they have survived the ordeal, but from the amount and character of their deflections. Every chain cable received into the Royal Navy is tested, and the action and effects of the test strain are exactly analogous to those of the hydraulic test for boilers. In France all wrought-iron boilers are required by law to be tested up to three times their working pressure by the hydraulic pump, and cast-iron boilers up to five times. So in America, all Government

boilers must bear a hydraulic pressure two-thirds greater than that at which they are intended to work.

Wrought-iron—following the same writer—it is true, elongates but from the $\frac{1}{600}$th to the $\frac{1}{2000}$th of its own length before the limit of elasticity is reached, and at this rate a boiler 12 ft. 6 in. in circumference would be enlarged only from $\frac{1}{12}$th to $\frac{1}{40}$th of an inch in diameter; but with even much less than this small alteration, the difference should be readily observable and measurable, with a thin steel wire placed around the boiler, and with its ends lapping over each other, the extension of the circumference of the boiler by from $\frac{1}{30}$ in. to $\frac{1}{4}$ in. would be very readily seen. That the test by the force-pump must be a useful one, it requires but little consideration to perceive; it may be safely, cheaply, and expeditiously applied; and there is no fear of the boiler bursting from weakness, original or produced, so long as careful measurement—both before and after the test—discloses no permanent distension.

In most instances of steam-boiler explosion but little difficulty exists in determining the immediate cause, although cases do occur in which no sufficient reason can be assigned. But there is a question underlying this which has, as yet, received no determinate solution at the hands of those whose disquisitions on the subject entitle them to be considered authorities. To what cause shall be attributed the violence attending a boiler explosion? To what agency shall be ascribed the partial demolition of a bridge by huge fragments of an exploded locomotive, as in the Rugby accident? How account for the flight of a boiler 40 ft. over a straw barn, as at High Moor Farm? or the snapping asunder of 14 in. solid cast-iron columns by a huge boiler launching itself forwards into a rolling mill many yards from its bed, as at the Midland Ironworks? Granted that the cause of explosion is correctly traced to the groove which, in the Rugby case, had been gradually eaten into one side of the middle ring of plates in the barrel of the boiler. The explosion of the boiler, thus weakened, would appear inevitable; and yet, an accident of frequent occurrence to steam-boilers is the mere rupture of corroded or otherwise weakened plates, or whole seams of rivets, but which accidents are generally harmless. But, then, they take place generally in the lower plates or seams of the boiler, well under water. If, in some cases, the local

weakness of a plate produces an explosion, why not in all, or nearly all, the general circumstances being the same? On the other hand, why should the rupture of any seam or plate produce a violent explos on at all? It is certain that the pressure of a jack screw within the boiler, pushing outwardly with the same force as the steam, would produce no explosion. It is equally certain that hydrostatic pressure, to the same extent, would merely open the plate and allow the water quietly to escape. Likewise, a boiler full of steam only, whether of 100 lb. or 500 lb. pressure per square inch, could never produce the wreck and ruin attendant on all violent explosions; there is not sufficient power in 150 cubic feet of high-pressure steam to account for these dire effects. Such a quantity of dry steam would, probably, have escaped harmlessly into the air before it could have imparted momentum to the heavy masses actually projected to considerable distances—there could be no exhibition of that terrible energy which characterises a true boiler explosion. The cause, then, of explosions is to be sought in some circumstance beyond the mere pressure of steam, the effect of which would rather be comparatively harmless rupture.

Mr. D. K. Clarke, in the *Mechanics' Magazine* in February, 1860 (which the author read), suggested the momentum of combined steam and water as being the cause of violent explosions. But perhaps no man of the present day has done more towards determining this question than Zerah Colburn. In a treatise, in which the subject is carefully investigated, he has elaborated Mr. Clark's idea, and has clearly and consistently disposed of all the arguments upon which were based the favourite hypotheses of superheated steam, electricity, explosive gases, spheroidal water, overpressure, and water freed from air, and has successfully established, upon perfectly tenable evidence, the indisputable fact that the violent explosion of steam boilers is mainly due to the combined percussion of the steam and water leaping up on the sudden removal of the steam previously resting on the surface of the water.

Taking, then, the views expressed by Mr. Colburn, it is to be observed, with regard to overheating, that although it is possible a boiler may be exploded by the formation of a great quantity of steam from water thrown upon red hot plates, overheating cannot be

assumed to be the general cause of explosions. Cases have occurred where a moment previously the gauges indicated an ample supply of water. In these, as well as in cases where there has been no positive evidence as to the amount of water in the boiler, the furnace plates have been found without any appearance of having been burnt. But assuming that water was suddenly thrown upon overheated plates, it is doubtful if the quantity of steam disengaged would be sufficient to increase greatly the pressure already within the boiler. In *The Engineer* of April 3rd, 1857, it is stated in a letter that an empty boiler, 25 ft. long, 6 ft. diameter, and with the safety valve loaded to 60 lb. per square inch, was made red hot. While in this state the feed was suddenly let on, and the boiler filled up. The experimenters were surprised at the result, which was simply a sudden contraction of the overheated iron, which caused the water to pour out in streams at every seam and rivet as far up as the fire mark extended. A consideration of some of the phenomena of heat leads to the conclusion that a red hot boiler may be as safely filled with cold water as a hot railway tyre may be quenched in a cistern. The metallic plates of a steam boiler are not capable of containing sufficient heat to change a very large quantity of water into steam. The total quantity of heat which would raise the temperature of 1 cwt. of iron through 1 deg., would, according to the best authorities, impart the same additional temperature to 12½ lb. only of water, and the quantity of heat which would raise the temperature of 1 cwt. of copper through 1 deg., would raise that of 10⅗ lb. only of water to the same extent. That overheating is not the sole cause of an explosion is clear, although it may lead to a rupture by weakening the plates.

With respect to the spheroidal state of water when thrown upon heated plates, it is observed that if ebullition were delayed until after a considerable quantity of water had been admitted, the heat of the plates would be so far absorbed in an equal or greater weight of water, that no explosion of the latter into steam could occur, thus rendering the spheroidal condition of water, under these circumstances, rather an argument against, than in favour of, the probability of explosion.

Many engineers hold that the presence of highly superheated steam within a boiler is sufficient to account for the most violent explosions

but neither by experiment nor by calculation has it ever been proved that such a result could arise from such a cause. It was the theory of Jacob Perkins (who heated steam out of contact with water to very high temperatures) that steam, being similarly superheated when the water in a boiler was low, the subsequent agitation of the water from any cause instantly produced a large additional quantity of steam, sufficient to cause an explosion. Another writer on the subject has assumed that ordinary steam, superheated to about 435 deg., would instantly convert water thrown among it into steam of a pressure of 360 lb. per square inch. But the fact must have been overlooked that 75 cubic feet of 140 lb. steam weigh but 26 lb., and that at ordinary temperatures the specific heat of steam is less than one-third that of water. Therefore, supposing the steam of a locomotive boiler superheated even to 350 deg. above the temperature due to its pressure, all the heat contained in 26 lb. of such steam could not generate much more than 3 lb. of additional steam, which would not raise the pressure at 140 lb. to more than 160 lb. to the square inch. Hence it is apparent that the conversion of water into steam, by being thrown up in a divided state, into highly superheated steam, is not sufficient of itself to account for any boiler explosion.

The electrical hypothesis was probably built upon the fact that electrical properties are sometimes developed by the discharge of steam into the air. But it would be impossible for electricity—if it were generated by ebullition or in confined steam—to collect within a boiler which is in direct communication with the earth. Professor Faraday's examination of Armstrong's hydro-electric engine, which produced electricity by the discharge of steam through a series of nozzles bushed with box-wood, afforded no evidence of electricity within the boiler, which was perfectly insulated. But he found that by using spring water instead of distilled water, as used by Armstrong, and by using other than box-wood nozzles at the points of discharge, no electricity whatever was obtained; and, further, that on discharging compressed air through box-wood nozzles the same results were obtained as from steam discharged under similar circumstances. If, as it has been argued, the boiler should become insulated by an internal coating of boiler scale, it would be necessary that the scale should completely cover every atom of the internal surfaces

of not only the boiler but its fittings also. But, perhaps, the best answer to the whole question is to be found in the fact that water may be boiled in a perfect Leyden arrangement, without the slightest development of electricity.

The decomposition of water by heat on a large scale, for the purpose of applying its elementary gases separately, has generally been attended with very unsatisfactory results. But this has not prevented the propagation of the fallacy that, under certain conditions, all the steam in a boiler is decomposed, and the liberated hydrogen violently exploded. This question was fully discussed by the author in a paper on superheated steam, in April, 1861, in which are included the elaborate reports of Dr. Taylor and Professor Brande, whom the patentees of a superheating apparatus had consulted; likewise that of Professor Faraday, who had been instructed by the Board of Trade to report upon the safety of the same apparatus. The question arose from the steam being superheated in pipes passing through the boiler furnace, in which case it was contended by some that the steam would become decomposed, and a violent explosion result. That steam is decomposed by being passed over red-hot iron is correct; but the iron absorbs and retains all the disengaged oxygen, and with the iron once oxidised decomposition ceases. And, further, hydrogen is not explosive, but simply combustible; and if even liberated, as suggested, its combustibility would not be manifested amid the enormous quantity of aqueous vapour liberated with and condensed around it. There is, therefore, no support whatever to the explanation of boiler explosions by the decomposition of steam.

An over-pressure is a pressure which exceeds the safe working pressure, and it may be generated either gradually or momentarily in a boiler; and, strictly speaking, there must always be over-pressure whenever a boiler is burst. When, however, an explosion is said to have occurred from over-pressure, it is generally understood that the pressure has been allowed to increase gradually up to the limit of the strength of the boiler, and if this has been calculated to correspond to a pressure of, say, 700 lb. per square inch, the actual pressure at the moment of explosion is accordingly assumed at that amount. Boilers may perhaps be generally capable of withstanding nearly their full calculated bursting pressures. But there are numerous instances of

the quiet rupture of steam boilers under ordinary working pressures, so that even a violent explosion does not absolutely prove that the pressure under which it occurred was anything like the calculated bursting pressure of the boiler. Suppose the bursting pressure to be 758 lb. per square inch, steam might be raised to that point and burst the boiler. But it is very improbable that anything like a pressure of 758 lb. ever accumulates in a boiler intended to work at 100 lb. or 125 lb. per square inch. If boilers burst only from over-pressure, they would of course give out first in the weakest part—say, along a seam of rivets. But after the seam had opened, the relief of pressure would be so instantaneous that, without subsequent percussive action, the rupture could hardly extend itself through solid plates of twice, or even ten times, the strength of the part which first gave way.

Repeating the original question: To what cause shall be attributed the violence attending a boiler explosion? Mr. Colburn, who has demolished the superficial theories of those who prefer mystery—or, at the least, obscure explanations, to circumstantial investigation, has not left the question there, but has raised upon no insecure foundation a theory well supported by science, and which effectually meets the proposition. His reasoning is conclusive. In all boiler explosions the pressure of steam is instantaneously liberated from the surface of the hot water present. If the boiler be at work at a pressure of 45 lb., the water must be at a temperature of about 290 deg. It is presumed to be sufficiently well known that fresh water cannot for an instant be maintained at a temperature much greater than 212 deg. under the ordinary atmospheric pressure. It may be true that, with great care and delicate apparatus, water may be heated to a little above this point without boiling, but, upon the least agitation, a portion will instantly flash into steam, and the temperature of the water will fall to 212 deg. For all practical purposes a body of fresh water cannot be kept a moment in the open air at a temperature above 212 deg., while, if the pressure upon it be liberated when it is heated to, say, 290 deg., a most violent disengagement of steam and projection of water along with it must inevitably take place. The force with which disengaging steam must carry water before it and along with it, must exceed any known force of the waves of the sea, not-

withstanding that the waves occasionally beat down some of the strongest constructions of man.

High-pressure steam rushes into the air with a velocity of .from 1500 ft. to 2000 ft. per second—a fact not only demonstrable from natural principles, but proved by the exhaustion within the fiftieth part of a single second of a cylinder full of steam in locomotives running at high speeds. The force with which it must hurl masses of water on all sides, like the fragments of an exploding shell, may be conjectured from this fact; and it is well known that, when moving with great velocity, a mass of water will strike with nearly the force of an equal weight of iron. For in firing a bullet from a gun down upon a surface of water—the concussion being necessarily identical with that which would result if the water were made to strike with the same velocity upon a stationary bullet—the bullet is flattened.

Inasmuch, then, as all boilers are constantly liable to rupture from original unsoundness of the iron, bad riveting, corrosion by bad water, or by the somewhat mysterious action of "furrowing," what is to be expected when, the steam pressure being thus suddenly liberated through a rent of moderate dimensions, 30, 40, or even 60 tons of heated water are waiting below to burst partly into steam?

It is clear, then, that there is always more or less danger in working boilers which are made to contain a great quantity of water. Safety lies in the use of numerous small water spaces, as in the duplicate retort boiler, the water-tube boiler, &c. In the latter class of boilers one or more tubes, where they happen to have been originally unsound, will now and then burst open, but so limited is the quantity of explosive water (as it may justly be termed when highly heated), that nothing like a violent explosion, disturbing the boiler as a whole, ever takes places in such cases.

According to Mr. Colburn, the distinct and consecutive operations into which a boiler explosion, although practically instantaneous, may probably be resolved, are, therefore:

1. The rupture, under hardly, if any more than, the ordinary working pressure of a defective portion of the shell of the boiler—a portion not much, if at all, below the water line.

2. The escape of the free steam from the steam chamber, and the

consequent removal of a considerable part of the pressure upon the water, before its contained heat can overcome its inertia and permit the disengagement of additional steam.

3. The projection of steam, combined as it necessarily must be with the water, with great velocity, and through a greater or less space upon the upper sides of the shell of the boiler, which is thus forced completely open, and perhaps broken in pieces.

4. The subsequent disengagement of a large quantity of steam from the heated water now no longer confined within the boiler, and the consequent projection of the already separated parts of the boiler to a greater or less distance.

Master minds have dealt so comprehensively with the subject of steam-boiler explosions, and it has been so well ventilated of late, that it is well nigh exhausted, and little, if any room, left for further comment. The present paper has no pretensions to originality. Taking advantage of opportunity, it simply proposes to separate fallacies from correct principles, and seeks to disseminate such opinions as are reconcileable with reason and supported by known facts. It was undertaken at a short notice, and draws largely upon Mr. Colburn's able exposition. An important conclusion, however, is clearly deducible—viz. that the general adoption of frequent and careful examinations, such as are made by the Manchester Boiler Association, together with the occasional application of the hydraulic test, would combine to effect a very considerable reduction in the number of fatal explosions, if it did not, indeed, wholly prevent them. Unless this course is adopted, the category of misfortune will go on swelling its dark pages until authority shall step in and, overriding obliquity, compel that which moral obligation ought to insure..

DISCUSSION.

Mr. E. Reynolds said the subject had been so fully treated in Mr. Nursey's excellent paper, that he did not think he could throw any additional light on the subject, but there were some few matters that might, perhaps, be usefully discussed. It appeared to him that it was more a matter of interest than of practical value, to trace the precise way in which the effects of boiler explosions were developed, though in general he had been more surprised at the smallness of the

results than the reverse. Of course, the prevention of explosion was the result to be aimed at, and he believed that there was no well-authenticated instance of explosion of a really good boiler, in good order, without some easily ascertainable cause. Such cases as have arisen from actual neglect, such as deficiency of water, or tampering with safety valves, or injudicious construction, and deficiency of stays, were sufficiently touched upon in the paper; he would, therefore, confine his remarks to those which care in attendance could not provide against, such as unequal expansion, and the most treacherous of all diseases that boilers were liable to—*i. e.* furrowing. He believed that danger seldom arose from the effects of unequal expansion, except where it induced furrowing without attracting notice. In its direct effect it was usually confined to producing injury and leakage from opening of seams, &c., rather than danger. The opening of seams in plates over the fires of Cornish boilers, and those of the under side of the shells of long double-flued boilers, if the cold water was made to impinge upon them, were common instances of its operation; but if the consequent leakage were allowed to exist long enough to cause extensive corrosion of the plates, then the danger came under the head of neglect. The same may be said of the effect of acids, &c., in the water used. It was scarcely likely that any water possessing sufficient corrosive powers to cause danger within the period that should elapse between examinations, could be used without its being suspected, if any care was taken in the matter at all. As to furrowing, which might generally be traced to the action of local alternating strains, operating in a manner which had been frequently explained, whether the local strain was produced by the steam in its endeavour to draw an overlapped seam into the line of pressure, or by machinery, or brackets fixed to a boiler, it was seldom so rapid as to render its detection difficult; but whenever the first trace of it is observed, the part should immediately be repaired, as the bending action of the strains which induced the furrowing would obviously go on in an increasing ratio as the plates got locally thinner.

He thought too much importance was attached to the hydraulic test for working boilers. It might be useful for ascertaining the absolute strength, and in new or repaired work was very useful; but

as a means of detecting local weakness, unless the weak parts were actually ruptured by the test, he thought it might be deceptive; for, as in cases of furrowing, the furrowed part might not exceed one inch in the 12 ft. circumference of a locomotive boiler—the excessive strain on this small part could scarcely be detected by the increase in the whole circumference of the boiler, though possibly the furrowed part might have been strained within one per cent. of bursting.

Another point to be remembered was, that to be of any utility the test must be much above the working pressure, and in the special case of locomotive boilers, they needed the test most after they had been some time at work, when the tubes were getting thin, and would not bear excessive pressure.

His own practice in examining boilers, was to test by hydraulic pressure all new or repaired boilers, and in other cases to drill holes wherever there were any indications of weakness, so as to ascertain positively what amount of strength was left for work.

He thought that there was too great a tendency at the present day, to use excessive pressures for large boilers for condensing land engines, where it was seldom productive of either economy or convenience. Where high-pressure was really required, as in locomotives, and other non-condensing engines, he thought a system of more careful supervision, and more frequent examination than had hitherto been prevalent, was highly desirable.

Mr. J. Lacey had heard that old boilers had been known to burst simply from the effects of electricity, which he thought might be accounted for by the insulation of the corrosion on the inside of the boiler. By the chemical action of steam, positive electricity was induced, which would produce an explosion in attempting to gain an equilibrium.

Mr. Olrick said that, with reference to this point Mr. Colburn, in his work on boiler explosions, had given Mr. Faraday's report, and the deduction drawn from that was, that no production of electricity was due to any change in the state of the liquid contained within the boiler, neither does the current of the steam produce any current of electricity. The electricity that could be produced, was only outside the boiler, and even in that case it could only be produced where there was a boxwood nozzle, and where that

nozzle was wet, the larger the pressure was the more friction there would be, and consequently the more electricity would be produced. As one of the professors had made careful researches, and had proved that it was impossible to produce electricity in boilers, they must bend to his opinion. The best engineers had not been able to prove anything against Mr. Faraday's researches. Mr. Colburn agreed so thoroughly with Mr. Faraday, that there was not a question about it. It was impossible to conceive that a violent boiler explosion could occur from any other causes than those given in Mr. Colburn's pamphlet, which was the most engineer-like explanation he had ever seen. It was explained by an engineer, and not a learned professor; and he should certainly, in a question of so much importance, sooner listen to the engineer than to a scientific professor, because there were many points which a scientific professor did not know, and consequently could not take notice of. He (Mr. Olrick) would offer a few remarks in respect to a recent boiler explosion noticed in Mr. Fletcher's report, recently published. The jury came to the conclusion that it was a boiler explosion owing to shortness of water. Mr. Fletcher, who was a boiler maker and engineer, came to quite a different conclusion. He said there had been no overheating of the boiler plates, and no injury from that cause. The boiler was 25 ft. long, the outer shell 6 ft. diameter, and the inner shell 2 ft. 3 in. The working pressure of that boiler was 70 lb. Having made a calculation, he (Mr. Olrick) found that the pressure at which it would collapse was 168 lb., that was if the boiler was new and there was no fault with the iron or the workmanship, and as they ought never to work with less than 6, 8, or even 10 for marine boilers as a factor of safety, every one would see that it was entirely wrong to work it at 70 lb., especially as the boiler was not new, so that it was easily explained why that boiler burst. The shell had a bursting pressure of 333 lb., and consequently the factor of safety was less than 5 instead of 6 or 8. There were many small points to be considered, which were often the cause of explosions. For instance, they would find many boilers where there were no proper back pressure valves between the feed-pipes and the boilers. In many cases of cheap boilers, delivered at prices of from 15*l.* to 20*l.* per ton, there was no such thing as a back pressure valve. In steam-ships, where there were donkey

pumps with fixed valves, moved by eccentrics, it often happened, instead of pumping water into the boiler, it was pumped out; it was therefore necessary whenever there was fixed, instead of loose valves, to be careful in starting these donkey engines. The safety valve was often made too small. The correct way was to make it in proportion to the coals burnt, or the heating surface. The safety valve ought to be so large that it could discharge easily the utmost amount of steam which the boiler could produce, and instead of one, have two safety valves; because if, as some maintain, boilers are burst by over-pressure, certainly they would be doubly safe; for if one safety valve should stick, it was not likely that both would stick at the same time. He did not mean to say that he maintained the theory that boilers were burst by over-pressure; but for the safety of those who took that view, it would be well for them to put two or three safety valves on. There was one thing which engineers ought to pay attention to, and that was the kind of water they pumped into the boiler; they should blow out three or four times a day, as they did in marine boilers. A boiler lately came under his notice which used to be blown out once in three months, and that was considered ample. The consequence was, he found half an inch of scale on the bottom of the boiler. The water used could be filtered, and the impurities would then be removed. Mr. Riley looked at him as much as to say that there were many things which a filter could not take away; but he (Mr. Olrick) had seen them within the last six months taken away by a process which was now in use, and which, perhaps, Mr. Riley was not aware of. Many hundred gallons of water could be purified in a day. He was sure if more attention was paid to the purity of the water, there would be a great deal less chance of boiler explosions from incrustation, because the consequences of impure water was incrustation. A great deal of sediment in boilers often choked up the different passages. Mr. Olrick then spoke of the construction of boilers, and showed by a diagram an example of a wrongly-constructed boiler that came under his own notice. The proper plan for introducing the feed-water was at the coolest place of the boiler, and not, as he saw it once in London, at the top of the fire-box. There was one kind of safety valve which had a great advantage over the usual kind. The valve was kept up by a column of water until the boiler

was entirely cleared of the over-pressure. It had been said that the usual hydraulic test was three times more than the usual pressure. That was wrong, it should never be more than twice, and in some cases he thought that would be too much. He could not agree with Mr. Reynolds in his objection to the hydraulic test. He (Mr. Olrick) considered it a great deal better to use a test if it was a careful one that would not injure the boiler. If there should be a faulty place, it would then be found out, and perhaps be the means of preventing a great destruction of life and property. The hydraulic test would not injure a boiler a bit more than a test under steam—some persons had objected because it was cold water—but they might warm it, make it boiling hot, and then try it by the hydraulic test, which would be just the same. Mr. Olrick concluded by illustrating by a diagram, a particular sort of plug used in boilers.

February 1*st*, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON "BOILER EXPLOSIONS."

BY PERRY F. NURSEY.

ADJOURNED DISCUSSION.

Mr. NURSEY requested permission to make a few observations previous to the discussion. He stated that since the last meeting at which he read his paper he had received a communication calling his attention to the fact that he had not given sufficient prominence to the part Mr. D. K. Clark had taken in the question of steam-boiler explosions, and directing his (Mr. Nursey's) notice to the *Mechanics' Magazines* of May 3rd and 10th, 1861, in which would be found a summary of the question up to that time. He had read the articles and correspondence referred to by the writer of the letter, and found that they embodied to some extent a difference of opinion between two gentlemen, which was a matter he could not with propriety

obtrude upon the meeting. Beyond this, the letters contained copious reference to various papers and articles, some of which were inaccessible, and which it was not possible for him to examine, which he felt bound to do before he could alter his views. With reference to Mr. Colburn's position in the matter, he (Mr. Nursey) had gone through the subject before writing his paper, and, taking Mr. Colburn's views from the earliest period, he would notice having observed the germ of the theory of steam-boiler explosions in articles written by Mr. Colburn in the *American Engineer*, in 1857, and before coming to England, and he had noticed its gradual development up to the time of the publication of the treatise by Mr. Colburn in March, 1860. It would be observed that the letters above referred to did not appear until May, 1861, more than a year after Mr. Colburn's treatise was published. In that work Mr Colburn gave Mr. Clark credit for several suggestions, and inserted a letter which Mr. Clark had written to the *Mechanics' Magazine* upon the subject. He (Mr. Nursey) would not trouble the meeting further upon a point which was somewhat irrelevant to the present question. He had only introduced it in fulfilment of his promise, and could not upon the insufficient evidence before him, alter the views expressed in his paper.

It had occurred to him, since reading his paper, that the method therein suggested of measuring the successive increments of strain upon boilers while under the hydraulic test, by means of a length of steel wire, was open to objection, and he hoped to hear from the meeting an improvement upon this suggestion.

Mr. Glynn said, at the last meeting Mr. Reynolds stated that he considered high-pressure boilers entirely wrong in principle, or that high-pressure boilers were not so good as low-pressure. Now, for his (Mr. Glynn's) part, he could not see any difference between them, provided they were equally well constructed according to the pressure they were intended to bear. He had been looking over some papers referring to several kinds of explosions, and it seemed to him that if they were taken in rotation, some conclusion might be arrived at as to the causes that led to explosions. In the first place, two or three of the explosions referred to in his papers, were entirely due to the want of safety-valve room. An explosion at Ashton-under-Lyne,

was traceable to the insane act of the stoker or engineer having prevented steam from escaping, by tying down the safety valve. There was a case in 1854, at Rochdale, where the boiler was 18 ft. long, 5 ft. diameter, $\frac{5}{16}$ in. plates, hemispherical ends, and part of boiler $\frac{3}{8}$ plates. This boiler should have been equal to a bursting pressure of 300 lbs. on the square inch. This, like many other boiler explosions, burst just after starting the engine, it having been standing for some time. The safety valves were weighted too heavily, and were also out of order. It seemed strange that these explosions occurred immediately after the starting of the engines; and the only way he could account for it was that as soon as the engine was started the steam valve was opened, and the rush of steam increased from all parts of the boiler. As the engine was at only $\frac{1}{2}$ or $\frac{1}{3}$ of its stroke, the valve was shut down, and the consequence was that a violent blow was struck on the boiler by the return wave of steam. His opinion with reference to the boiler at Rochdale, was that the ordinary pressure of steam (76 lb.) was too much for a boiler, the supposed bursting pressure being 200 lb. Another cause of boiler explosions was the incrustation which was formed along the top flue, and often in the bottom flues: he was speaking of land boilers. That boilers did get incrusted, more or less, was certain; and in some cases, to a large extent, and was a fruitful cause of boiler explosions. He cited the case of the explosion on board the *Malaga*, and read an extract from a description of it by Mr. Hick. It was to the effect that a very thick incrustation of salt had been formed on the lower part of the boiler, immediately over the fire. He (Mr. Glynn) thought that this incrustation being almost a non-conductor of heat, or absorbent, would absorb as much as 50 to 75 per cent. According to Mr. Bridges Adams, the plates under the incrustation were very liable to get red hot, thus weakening the boiler, if only by continually overheating them. But there was more than this; the expansion of the boiler plates and the incrustation being unequal, the incrustation breaks up, and suddenly brings water on the red-hot plates, and at least produces that extra amount of pressure in the boiler which the plates cannot bear, having lost $\frac{5}{6}$ of their strength by this heat. This was borne out by Dr. Ritterbrandt, who says "that a sudden evolution of steam under circumstances of incrusta-

tion, is no uncommon occurrence." Another cause of boiler explosions was deficiency of water, which caused the plates to come down over the fire and to leak, and, consequently, rendered the boiler very liable to explosion. Another case where the water could not be properly ascertained was in boilers that primed a great deal, which was more especially the case in locomotives and marine boilers. The steam gets into the water, and then there was registered in the gauge a larger amount of water than there really was. He had not heard anything said about the spheroidal condition of water, or explosions arising from that condition. With reference to this, he would read an extract from a report on the bursting of the Union Steam-Packet boilers, written by Mr. Pearsall, a pupil of Professor Faraday, wherein it is said, "I consider the immediate cause of the bursting of a boiler from the expansive power of steam, and decidedly not from gas," &c. &c.

He (Mr. Glynn) called attention to a case which might occur to an engine that had been standing for some time, and where it was usual to blow through before starting. In blowing through, the water was thrown out of the hot well through the blow valves, if it had not leaked previously, through the valves themselves being out of order. When the engine was started, until the passages to, and the air pipe as well as the hot well was full, and the whole of the passages covered completely up to the feed pump, air would be directly pumped into the boiler; and if the valves leak, the whole connexion from the boiler and the force pump would be charged with air: consequently, there would be two reservoirs of air. He could not positively say there would be sufficient air to cause an explosion. This makes a boiler very liable to explode by its generating an amount of dry steam; and although Mr. Colburn thought there was no elasticity, he (Mr. Glynn) believed there would be a great deal of elasticity at that temperature. At the last meeting mention was made of a plug being employed. He had made a sketch of a fusible plug made by Mr. Forsyth, of Wolverhampton, in 1854. This plug was made of a large brass bolt (in which the fusible metal was soldered), capped on the fire side, so as to bring the lower side of the tin or lead plug within it, some distance above the water-side of the roof plate, thus leaving a margin, and allowing the plug to be fused while there was

sufficient water upon the plates to prevent their being burned, and to drown out the fire after the plug had been fused. In France, the use of a fusible plate (made of lead, tin, and bismuth) let into the boiler, was found advantageous, as it was out of the power of the engineer or stoker to interfere with it. Another plan proposed was to have a small tube projecting out of the top of the boiler, and screwed into a frame, so that if there was any extra pressure in the boiler, the tube would blow out with a report, thus giving notice that attention was required. There was another point with regard to water gauges; these did not always tell exactly the quantity of water that was in the boiler. Some were so badly constructed that, instead of the water working on the bottom, it was really the reverse. Another probable cause for the production of boiler explosions was this: In large towns space was very valuable, and when a manufactory was started, a boiler and engine were erected just large enough to do the work required at the moment. By-and-by, when the work increased, there would be no spare room to put up another boiler, and the consequence would be that an additional duty must be put upon the one boiler, when it was least fitted to bear it. He believed that to be one of the strong causes of boiler explosions. Another cause, especially in some works, arose from the boilers not being covered; the result was, that oxidation took place on the outside of the boiler, which caused it to scale, and thereby to become weaker. The grand secret in preventing boiler explosions could be thus summed up: First, the boiler should be properly constructed in its shell, and good, not cheap, materials used; there should be *two* safety valves, so constructed that the engineer or stoker could not tamper with them; there should be proper feed apparatus, self-acting, and to be worked by hand; and also large water gauges and gauge-cocks. With these precautions, and a proper engineer and stoker, explosions need not be feared. When he said "a proper engineer," he meant one sober and well conducted, who considered it a necessity to have his safety valves working easily, and kept clean, attention to the gauges, use of fire-plug and feed apparatus. When such men as these were employed (and they could be obtained, but not at 18*s*. or a 1*l*. per week), it might surely be said that everything had been done to prevent boiler explosions. He concluded by saying that he considered the chief

engineer as acting criminally when he neglected to look to these points; and that he hoped yet to see the day when it would be made legally a criminal matter when those conditions were not attended to.

Mr. Z. Colburn said, the most conclusive evidence of the soundness of the theory which had been referred to would be suddenly to condense steam in the steam chamber of a boiler at work, and see what results would take place. It might be attended with some danger, but he thought it might be managed. Supposing the boiler to be half full of water, and the steam up to 30 lb. or 40 lb., it might be reasonably expected that the strength of the boiler would be equal to 200 lb. If a quantity of cold water were suddenly thrown into the steam space, the steam would be suddenly condensed, which, probably, would immediately produce an explosion of the boiler. He (Mr. Colburn) proposed trying the experiment at some future time, and he would lay the results before the Society. The question did not appear to have been sufficiently considered, why did boilers explode so violently? A vessel, like a boiler, but filled only with steam, or compressed air, produced no such effects as were produced in boiler explosions; but the bursting of water into steam would tear open the boiler with a loud report, and the pieces would be hurled eighty or a hundred yards, or more. He believed that boilers quietly ruptured, without violently exploding, veiy much oftener than they exploded; but such cases were seldom heard of, because nobody was injured. He had been twice upon locomotives when the whole side of the fire-box, perhaps a foot below the water line, opened instantaneously. Of course, these might be called explosions, but still the engines were not thrown off the rails. During the last year a fatal case of rupture was reported in the newspapers. It was a consideration of the above facts that led him to investigate the question why water should explode violently, and he found no other reason than the sudden removal of steam pressure. With regard to the remarks made by Mr. Glynn, relative to the shock communicated through steam, it seemed to him (Mr. Colburn) that the theory was not correct.

Mr. Danchell considered that Dr. Clark's process of precipitating lime in solution, which had been alluded to at a previous meeting,

and the invention of which had wrongly been attributed to him, did not go far enough in removing salts out of water which had to be used for boiler purposes. Carbonate of lime was thrown down by Clark's process, but not the sulphate of lime; and it was the latter which principally caused the hardness of those incrustations in boilers. It appeared to him desirable that all salts, and, in fact, all impurities, in water should be removed from the water before entering the boiler. There were many who believed that boiler explosions often arose from the incrustations. Whether or not this was the case he was not prepared to say; but one thing was quite certain, and that was, that they did not lessen the chances and could do nothing to *prevent* explosions; and, therefore, as they were injurious in many other repects, they ought to be prevented altogether. The only way of effecting this was by preventing the salts, which occasion the incrustations, from entering into the boiler. Carbonate of lime, in precipitating, formed a loose powder, but sulphate of lime a hard crust. Both together would likewise form a solid incrustation, more or less hard in proportion as the one or the other of these two were predominant. He exhibited some specimens of water containing the different salts in solution, and also some experiments of their mode of precipitating. He pointed outthe fallacy of estimating any water from appearance. He showed a specimen of the water from Red Lion-square pump, containing 140 grains in the gallon of different kinds of salts; that from the Thames—considered to be bad enough—contained at most 20 grains, and yet the first appeared much purer than the latter. The water in many places used for steam purposes was even worse, and the great inconvenience, loss of time, and waste of fuel, ought to be inducements enough, independent of what possible danger may accrue, to adopt proper means to purify the water before letting it into the boiler.

Mr. Carrington almost entirely coincided with Mr. Colburn's views on boiler explosions. He thought Mr. Glynn had partially misunderstood Mr. Colburn, for the latter had said, that in many cases boilers were burst, or torn open, without any violent or loud and dangerous explosion. He (Mr. Carrington) admired Mr. Colburn's theory very much. It was self-evident that a large amount of water would be carried by the steam as it expanded in the water on the

pressure being relieved, and, of course, the force of the blow would depend upon the velocity with which it struck. Assuming a quantity of water under 15 lb. pressure of steam, about $\frac{1}{40}$th of such water would change into steam on the pressure being relieved; and if the pressure was relieved instantaneously, the velocity of the mass of water and steam would be great and very destructive. A blow from this mixture would be far more severe than from the same weight of steam alone; for the mixture having only about $\frac{1}{40}$th of its weight of steam in it, would have far less elasticity than if it were all steam.

Mr. F. Young thought that where the steam had become surcharged with heat, and the water mixed with it, the water became steam, and added to the pressure in the boiler. Mr. Colburn had referred to the effect produced by throwing cold water into the steam in a boiler. This was done a long time back by Mr. Brunel; but, as far as he (Mr. Young) had been able to learn, no result whatever was produced. Allusion had been made to the blow of the steam against the boiler. He recollected the case of the *Parana* steam-ship; and here the whole force of the explosion seemed to be exactly opposite the steam-pipe. He certainly considered it very important that all boilers should be subjected to hydraulic tests; filling the boiler with water, closing the valve, and lighting the fire, would be a very much fairer way of testing, and certainly cheaper than by the pump. With regard to incrustation, he recollected in the steamer *Dart* the engineer allowed the boiler to become salted, and about an eighth of an inch thick of salt was found in it. The result was the roof of the furnace came down; but, owing to the goodness of the plate, there was no explosion. The furnace had to be taken out and restored to its original from, and the steamer was now running. It was most necessary that there should be a reliable inspector of boilers. A man must not make a gun, or sell it, without having it proved, under a penalty; and why should it not be the same with boilers? He certainly could not see the wisdom of interfering with the gun and not interfering with the boiler. In France all boilers were proved by Government. He agreed with Mr. Glynn that boilers should not be placed in inefficient hands, but that they should be managed by efficient engineers only. In France a man must pass an examination before he is considered competent to take charge of a boiler. With the

manufacturers of boilers, at the present time, it was not the question of quality, but that of cheapness, for boilers were now supplied at a price little more than the cost of the materials. As regarded the statement that revolving boilers gave more steam, he thought it seemed reasonable enough; and the motion must be of very great assistance to the circulation of the water, which should certainly be attended to, but which seemed to be very much neglected.

Mr. Riley stated that it was a well-known fact that boiler explosions frequently took place at the starting of the engine. This might be explained by the fact that in suddenly withdrawing the pressure in the boiler a violent ebullition took place, and caused a great evolution of steam from the water being thrown upon the heated plates, if the water in the boiler had been allowed to get at all low. He also referred to the violent explosions that occur occasionally in iron-works, by the use of water for removing the cinder from refined metal. After the molten metal is run in the moulds cold water is thrown upon it with buckets. This operation is safely performed. If, however, there is the smallest particle of iron or cinder in the water thrown on the metal, then a violent explosion ensues, scattering the molten iron in all directions. The same effect is produced when cold water is poured into a puddling furnace, to cool the bottom. If the water contain a particle of iron or cinder the furnace is not unfrequently blown down altogether. It certainly was very difficult to explain the reason of this. It might be due to the cold piece of iron causing contact, as it were, between the hot metal and the water, by which steam is suddenly generated; whereas, all the other water enters into the spheroidal state. In boiler explosions it was probable that cold water being introduced in a boiler with red-hot plates reduced their temperature, and when the water came in contact with the iron a sudden evolution of steam would be the result, which might account for the boiler giving way.

Mr. Louch stated that several instances had come under his observation, in which corrosion had occurred from the boilers having been set with a line of rivets resting immediately upon the brick-work setting. Any leakage from this line of rivets would settle between the boiler and brickwork, and the moisture thus—assisted no doubt by the sulphurous vapours generated in the flues—caused

very rapid corrosion, and probably led to many explosions. He therefore recommended that great care should be exercised in setting boilers to avoid as much as possible a line of rivets coming in contact with the brickwork. He also strongly condemned the too prevalent practice of entrusting boilers to inexperienced and underpaid attendants, to which in his opinion many boiler explosions might be attributed.

Mr. Olrick referred to a plug of quite a different character to that to which allusion had been made. He considered it a superior one, and for this reason, that this plug would act while there were still 2 in. of water over the boiler plates, consequently the plates could not be injured by overheating. When the water was too low the soldering with which it was soldered would melt away, and the top part would by the force of steam be blown into the fire, when the fireman would be woke up, if he should happen to be asleep, by the velocity with which the steam rushed out, besides that, if he did not attend to it at once, he would have the fire out. Mr. Glynn had mentioned that it was possible, under certain circumstances, that there would be a great deal of electricity in a boiler. He (Mr. Olrick) thought they ought to rely upon the opinion of Professor Faraday, who says, "The production of electricity is not due to any change in the state of liquid contained in the boiler, a current of dry steam produces no development of electricity." If that was the opinion of Professor Faraday, he considered they should conclude that it was impossible for any boiler to explode simply on account of electricity, and consequently all those who brought that forward as one cause of explosion, should at once consider they were labouring under some mistake. Mr. Glynn had stated that one of the professor's pupils thought differently from the professor upon the question of decomposition of steam. As Professor Faraday considered that decomposition could not occur to any extent under any circumstances in the working of ordinary steam boilers, and that if it did occur the hydrogen thus liberated would have no access to oxygen, without which it could not inflame or explode, even if oxygen were present, steam would prevent ignition; and if oxygen were present and no steam existed, the hydrogen would only inflame and burn silently, as fast as it was produced. Consequently, explosion was impossible from decomposition.

If, he said, the master held these views, and had come to this conclusion, he thought they were bound to accept the views of the master in preference to those of the pupil. He quite agreed with the remarks that the circulation of the water in boilers was much neglected. He believed the want of proper attention in this respect had caused many explosions. Mr. Young had referred to an incrustation of salt having been formed upon a boiler; that ought not to have happened, for it could have been prevented by using a proper salinometer. As regarded high-pressur eboilers, he thought they were the safest, not only with respect to pressure, but also with respect to the prevention of accumulation of deposits, because the circulation of the water was increased. There was another point to which no reference had been made. Professor Miller had made experiments which had thrown considerable light upon some of the causes by which ebullition was facilitated. He found that the presence of air singularly assisted the evolution of vapour. That was an easy explanation of the many explosions shortly after starting the engine, because the pumps would first pump air into the boiler. There was another point which he thought ought to be mentioned, as it had been published. He referred to water that contained organic matter producing inflammable gases. As to the management of boilers, it was of as much importance to have good men, as it was to have good boilers; and let them be properly attended to, and let the water be cleansed as it came into the boiler by having proper scum-pans. It was an easy matter to prevent the impurities entering the boiler, by filters, or some other means, such as Dr. Clarke's process, which throws down the carbonate of lime, but retains the sulphate. He had seen two tubes taken out of a boiler where sulphate and carbonate of lime and salt had formed into a solid mass, and he had seen an eighth of an inch of salt accumulate in a voyage from Sunderland to London. Mr. Olrick considered that zig-zag tubes would be difficult to clean, and that they would materially interfere with the circulation of the water.

Mr. E. Reynolds rose to explain some remarks made by him at the last meeting, which appeared to have been misunderstood, and adden, that with reference to the part performed by the water in the act of explosion, it was only fair to Mr. Craddock to point out that he

appeared to have fully understood, and published what was perhaps put in a clearer light by Mr. Colburn, but could not now be claimed as new. In his lecture, published in 1847, fol. 30, this passage occurs:

"As we, then, see the part the 9 deg of sensible heat contained in in the 160 cubic feet of water play, on the latter varying only 9 deg. in temperature; if we, then, suppose steam at 40 lb. on the inch, which on the bursting of the boiler is instantly reduced to 20 lb. per inch, including the pressure of the air, we shall find here that we have 40 deg. difference of temperature, and that as much water would instantly flash into steam as would produce upwards of 10,240 cubic feet: this explains the diffusive character the water on such occasions assumes."

And at fol. 23: "But supposing it were to burst, it contains nothing but steam, which is in itself not a very dangerous element, the chief danger in boiler explosions arising from the great quantity of water, together with the solid matter which is blown about in all directions."

Mr. Shrubb said he had been inspector of boilers at the Royal Gun Factories at Woolwich for three years, and his experience told him that compositions to prevent corrosion did not answer when there was so much salt and hard silica substance contained in the water, as was the case at Woolwich with the water taken from the Thames, although it would answer where boilers were supplied with purer water. He found that by regularly blowing out, boilers were kept much cleaner than with the scum-cock. The composition, he found, only took off the lighter products of the sediment, and left the heavier and hard silica portion, which, though much thinner, was impenetrable to water, but if the two were kept to-together they formed a porous body which allowed the water to reach the plates.

As regarded the effect of cold water thrown upon a heated plate, he had seen a plate ripped right through the centre of the rivets from cold water being thrown upon it.

With respect to safety valves, he had adopted Hopkinson's compound safety valves throughout the Gun Factories, as they acted inside the boiler, and could not be tampered with without taking off the man-hole door. If the attendant neglected to keep a sufficient

supply of water in his boiler the valve would open, and in three or four minutes the boiler would be empty of its steam and his engine would stop, and there would be no steam to work his pump to put cold water on the tubes of his boiler; consequently, it could be immediately proved that it was the entire neglect of the attendant. In fact, explosions would never occur if the boilers were properly examined and attended to.

To facilitate the process of cleaning the boilers, Mr. Shrubb said that he had the refuse oil from the drip cans of the shafting collected, and after the boilers were cleaned this oil was put on the inside of the boiler with a brush; although it did not stop the incrustation it made it come off the plates much easier, and effected a great saving in time and expense in cleaning the boilers.

He considered the mushroom valve, so much in use now as a safety valve, should be done away with entirely, and flat surface valves substituted in their place, as the mushroom valves were continually getting jambed from dirt and expansion from heat, which was not the csse with flat surface valves.

Mr. Nursey, in reply, observed that a great many points had been raised during the discussion by some members, but which had been so completely answered by others, that it would be unnecessary for him further to notice them. He thought, however, that distinction had not been sufficiently drawn between the cause of steam-boiler explosions and the cause of the *violence* attending them. The object of the paper was to enquire into, and as far as possible to determine, the *cause* to which this violent action was attributable. It had been shown that the percussive force of steam alone, was incapable of pro ducing the destruction attending most steam-boiler explosions. Overheating the metal of the boiler could not be assumed to be the general cause, for explosions had often taken place where, but a moment before, the water gauges indicated an ample supply of water. A boiler had even been filled while red-hot with water, but no explosion took place. The electrical hypothesis had been proved untenable. That the decomposition of steam was a cause of the violence —or even of explosions at all—had been negatived upon the highest authority. The legitimate solution to the question, therefore, appeared to be found in Mr. Colburn's views, which were, in effect,

the rupture of a defective portion of the boiler shell—the escape of steam, and consequent instantaneous reduction of pressure upon the water—the rapid disengagement of steam and its projection in combination with water through a greater or less space, and the conversion into steam of part of the heated water thus projected. Practically, these operations were instantaneous, and to them was clearly due the resistless energy of steam-boiler explosions. There could be no question that periodical examinations and the occasional application of the hydraulic test would go far to prevent steam-boiler explosions, but, above all, a careful supervision was necessary over those in charge, who should be properly qualified.

The CHAIRMAN thought they were greatly indebted to Mr. Nursey for his judicious and well-considered paper, and for the manner in which he had laid the subject before the meeting. The discussion had resolved itself into this:—the general position and nature of the fracture of exploded boilers, and the causes of these explosions. He considered it to be their especial duty to attend to the different causes of explosions. Further scientific research was very desirable, so as to enable them to come to some satisfactory conclusion upon the extraordinary phenomena observed in terrific explosions. The whole of the discussion (he thought) led to the opinion that Mr. Colburn's theory bore greater evidence of truth than any other they had discussed. Mr. Colburn had told them that he intended to practically test the truth of his theory upon a small scale; and he (the Chairman) hoped he would conduct the experiments with safety, and not blow himself up as well as the boiler he was experimenting on. (Laughter.) The Society would be very glad to know the result of these experiments when finished. A strong opinion had been expressed that there should be a Government intervention with regard to a proper supervision of boilers. One gentleman mentioned that Government exercised such a supervision over fire-arms. He was not aware himself whether this was so or not, but if Government did interfere with fire-arms, there could be no doubt whatever that there was a strong reason why they should exercise the same control over steam boilers. And who, he would ask, were the proper persons to bring such a subject before Government for consideration? Was it not the duty of such a society as the Society of Engineers? (Hear,

hear.) If they did he thought there was every reason to believe every careful attention would be given to the subject; and did not this proposed control interfere with any general principles, he thought a strong probability existed that any such representations made from the Society might result in a permanent benefit to the public. He should certainly be glad to see the Society memorialise Government on this subject.

March 2nd, 1863.

W. T. CARRINGTON in the Chair.

ON THE RELATION BETWEEN THE SAFE LOAD AND THE ULTIMATE STRENGTH OF IRON.

By Zerah Colburn.

A great number of experiments have been made by many experimenters to ascertain the ultimate resistance of iron to tension and compression, and its strength has thus been determined with perhaps as much precision as is possible in the case of a material presenting almost constant variations of quality. Every engineer is now aware that, as an average result, the tensile strength of good cast-iron may be taken as about 8 tons per square inch and its crushing strength as 48 tons. Wrought-iron of fair quality will bear not far from 22 tons per square inch in tension, while its crushing strength is variously stated at from 12 or 15 tons per square inch up to 28½ tons, the last named being given by Mr. Mallet as the result of experiments upon large hammered bars which bore but from 23 tons to 24 tons in tension.

When, however, we come to the question of safe working strength much difference of opinion exists among engineers, the permanent supporting power of iron being variously estimated at from four-tenths down to one-tenth of its breaking strength. Thus, when some fifteen years ago, a royal commission sat to inquire into the application of iron to railway structures, the late Mr. Glynn, in his evidence,

recommended that a cast-iron bridge should never be loaded beyond one-tenth of its ultimate strength. The late Mr. Stephenson, with several other engineers, thought a ratio of one-sixth sufficient, while the late Mr. Brunel was satisfied with a ratio of from two-fifths to one-third; or, in other words, if a girder would just bear 100 tons of distributed load, he would put from 33 tons to 40 tons upon it, where Mr. Stephenson would allow not more than 17 tons and Mr. Glynn only 10 tons.

Were we now to have another commission entrusted with the same inquiry, it is not unlikely that as great a difference of opinion would be found still existing. For there is no acknowledged natural principle upon which the safe load of iron has yet been determined, and in the absence or oversight of such a principle each engineer must be governed by his own judgment of what is safe and prudent. It is true that the authority of the Board of Trade has been so far exercised in this matter as to have limited engineers, in the design of wrought-iron railway bridges, to maximum tensile strains of 5 tons per square inch; and although it is commonly believed that with wrought-iron a compressive strain of from 4 tons to 4½ tons corresponds to a tensile strain of 5 tons, the Board of Trade impose the same limit of strain for both the top and bottom chords of a wrought-iron girder. The limit of 5 tons per square inch, it is hardly necessary to say, is an entirely arbitrary one, nor is it modified according to the quality of the iron and workmanship in a structure. Thus, in girder bridges, plate-iron is used of which the breaking strength is occasionally not more than 18 tons per square inch. In punching the rivet holes, however, and irrespective of the loss of the metal actually punched out, the solid iron remaining between the holes is injured, so much so, that in a series of experiments made many years ago by Mr. Fairbairn, the mean tensile strength of seven specimens was reduced from 52,486 lb. per square inch before punching to 41,590 lb. per square inch of solid iron left between the holes after punching—more than 20 per cent. of the strength of the iron being destroyed by punching, a loss distinct from that of the metal actually punched out. Drilled rivet holes, it is satisfactory to know, are now being adopted in the best class of bridge work, but in bridges already erected and containing plates occasionally no stronger than 18 tons

per square inch before punching, the loss of strength ascertained by Mr. Fairbairn would diminish this to about 14½ tons for the net section of metal between the rivet holes. On the other hand the best suspension bridge links have a strength of from 26 tons to 28 tons per square inch of section, and yet the Board of Trade inspecting officers would not probably depart from the arbitrary limit of a maximum strain of 5 tons in either case. As far, therefore, as is necessary to meet the requirements of the authorities, good iron and sound workmanship go for little or nothing; and not only does this remark apply to the interference of the Board of Trade, but, in the case of Chelsea Suspension Bridge, the chains of which are believed to have a tensile strength of upwards of 25 tons per square inch, two of the leading members of our profession have declared that structure to be unsafe until it shall have been so strengthened that the greatest load which the heaviest traffic is likely to bring upon it shall not exceed 5 tons per square inch of the sectional area of the chains. The highest authorities, we are justified in supposing, would, at the same time, be satisfied with the same maximum strain in the chords of a plate girder bridge even if the actual breaking strength of the solid iron between the rivet holes did not, as we have reason to believe it often does not, exceed 15 tons, or three-fifths that of the links of Chelsea Bridge.

While some of the conditions of elastic action are now under consideration, it appears desirable to define, as nearly as may be, what elasticity really is. We are all familiar enough with its manifestations, as in a watch-spring, an air cushion, or a bit of india-rubber, but we seldom inquire to what condition of matter these manifestations are to be attributed. When, however, we regard matter of any kind as a collection of distinct molecules or atoms, between which the force of attraction is always predominant in solids, and the force of repulsion in gases, we have no difficulty in perceiving that any alteration in the amount of either of these forces, acting within a body, must produce a corresponding change in the distances between its atoms, and in this very change of distance we have the whole phenomenon of elasticity. Elasticity is not a force, but the result of the antagonism of two opposite forces: an antagonism in which either force continues to act through a certain range or distance

—a range differing greatly in different materials—before being completely overcome by the opposite force. Under the influence only of attraction the particles of all matter would cohere, and without repulsion there could be only solid bodies in nature, for the existence of liquids and gases would then be impossible. The repulsive force appears to be the force of heat, to which physical repulsion may be generally traced. Under the influence of heat alone, and without attraction, there could be neither cohesion nor liquidity, and all substances could exist only in the gaseous state. We shall soon be able to trace the bearing which these distinctions have upon the elasticity of iron. At present we have only to bear in mind that there is reasonable physical evidence to show that the atoms of matter, so far as we can comprehend their existence, are never in actual contact with each other, even in solid bodies. Iron, as we know, is a comparatively porous material, since air and water may be readily forced through it under moderate pressures. Mercury, it is well known, will rapidly insinuate itself between the pores of an apparently solid body, for gold immersed in mercury will acquire the condition of an amalgam, and will crumble to powder. We know, furthermore, that the particles of most, if not all, solid bodies may be so far separated by heat as to convert them into liquids, and it is quite reasonable to suppose that, with an additional degree of heat, or repulsive force, all these liquified solids might, like mercury, and even melted gold, be converted into vapours. Now, any force applied after the manner of tension in a bar of iron, thus tending to separate the particles, in opposition to their cohesion or force of mutual attraction, may be considered to act in the same manner as the repulsive force of heat. The point at which rupture will take place will evidently be where the particles are so far separated that the cohesive force is less than the tensile strain, but there is good ground for supposing that, even after rupture, the atoms of the iron would again cohere could they be again brought to within their original distance from each other. This, of course, would be impossible if we attempted merely to place the two fractured surfaces in contact. In the case, however, of two clean surfaces of lead, we may readily bring them so closely together as to restore cohesion, and in the same way iron journals running in steel bearings occasionally become welded to them when the pressure

is too great for ordinary work. In thus bringing two bodies within the range of cohesive force, we must remember that it is the ultimate atoms which cohere, and that the finest perceptible particle of matter may really consist of myriads of almost infinitely smaller atoms. In all welding we first employ a degree of heat sufficient to overcome so much of the cohesive force between the atoms of the iron as to allow of sufficient motion among themselves to bring all or most of the atoms forming one surface within cohesive range of those forming the opposite surface. Two cast-iron surfaces are readily welded together by running melted iron between them. Without heat the two surfaces, however well finished, would touch only at a great number of separate points, although there would be considerable cohesion even then, as every one knows who has brought two carefully scraped surfaces of iron or two sheets of plate-glass together; and although a part of the apparent cohesion is due to the pressure of the atmosphere, there would really be some cohesion in a vacuum. It is only from the fact that the atoms of matter will always cohere when once brought sufficiently near together, that we are enabled to make bricks and all articles of earthenware. Soluble bodies, like clay or salt, cohere after solution and evaporation simply because their atoms, having been first separated by the liquid solvent, are again left within cohesive range on the evaporation of that solvent. Bricks, but for the waste of fuel in burning them, might be made from the most dilute solution of clay, and the final baking is only necessary in order to so completely expel the moisture as to bring the clay atoms within the range of cohesion, but their own character is in no respect altered. Iron, where it possible to dissolve it without combining it with nitrogen, oxygen, sulphur, or other matters, would again return to its ordinary granular state, and would recover its cohesion, on the evaporation of the solvent. Great pressure, too, will effect cohesion in the cases of substances naturally having none at all. Thus in experiments made with the ballistic pendulum to ascertain the velocity of cannon shot, the sand packing against which the shot is fired is often converted into sandstone of considerable strength.

From the illustrations given the elasticity of solids appears to be no more than the range or play of the attractive and repulsive forces

of matter, as variably exerted, but within the limits of rupture or crushing. Thus elasticity is the same in kind whether the repulsive or separating force be externally applied, or whether it be that of heat acting between the molecules of the body. If a bar of good wrought-iron be stretched to the one-thousandth part of its length, corresponding, say, to a strain of ten tons per square inch, its elasticity will be fully excited or nearly so, and it will not support a much greater strain without taking a permanent set. It is true that, if the same bar of iron, when not under strain, be heated to from 150 to 200 deg. above its normal temperature, it will also elongate by one-thousandth part of its length, and that without injury. But if this elongation take place under a compressive strain, or, if the iron, first raised in temperature by 200 deg. and thereby elongated, be attached to two fixed points, and thus, while cooling, be made to contract under strain, it will be found that an elongation of not far from one-thousandth of the original length of the bar is the most that can be borne without injury, even when that elongation is due to heat alone. But if the iron be first heated sufficiently to soften it, as railway tyres and gun hoops are heated, the particles will be re-arranged and, within certain limits without injury, but after the metal has once cooled below the temperature at which the particles have the mobility necessary for this re-arrangement, any further contraction around an unyielding object will be attended with permanent and, there is reason to believe, injurious strain. Even in setting railway tyres, it is believed to be best to put them on cold, and under graduated pressure, and in the case of gun hoops, Captain Blakely and Mr. Mallet, who appear to be entitled to the credit of the modern system of the ringed construction of artillery, having always insisted upon the importance of a definite degree of shrinkage of each ring, so that the consequent strain shall not exceed the elastic limits of the material. Sir William Armstrong has stated that he does not consider any especial accuracy essential in the distribution of the strains imparted by shrinking his gun hoops upon each other, but it may be questioned how far the failures of so many of the Armstrong guns have been due to neglect in this respect.

It would be interesting to know the precise manner in which a separating strain acts upon the molecules of iron, or rather to know

the successive positions of the atoms during the application of the strain. We are, however, without any positive knowledge of the positions which the atoms assume in solidification and under subsequent forging, but the multifarious forms in which all atoms visibly crystallise serve to show us that they cannot all be at equal distances from each other throughout the whole body. If they were, the arrangement would be that of cannon balls in a triangular pyramidal pile. Could we visibly represent the atoms, as occupying the angles of an infinite number of equilateral triangles, we should understand that a linear strain acting to separate any two of the atoms would, at the same time, draw a third atom, if not a number of atoms partly between them. And when, from this intrusion, the repulsive force, or heat, always enveloping the intruding atom, had once overpowered the attractive or cohesive force existing between the two atoms thus strained apart, these would, in turn, cohere anew to the atom which had been drawn in between them, and thus we should have a permanent re-arrangement of the atoms, or, in other words, a permanent set, with permanent elongation in one direction, and permanent contraction in a plane at right angles thereto. That the atoms are thus drawn into parallel rows of straight lines, in many kinds of iron at least, seems evident from the appearance of fracture, which presents stringy collections of particles forming what is commonly called fibre, although there is great reason for doubting that anything like fibre existed in the iron before it was broken. Mr. Kirkaldy's recent extensive experiments appear to show, as many others have shown, that iron may be made to break short or to break with an appearance of fibre, just according as it is broken with a sudden blow or a gradual pull. Something like fibre may be imparted, on a coarse scale, by repeated rolling or in wire-drawing, but it is more probable that masses of atoms are thus drawn into strings than that any fibre is really imparted to the atomic arrangement itself.

It is commonly held that, within certain limits of strain, iron is perfectly elastic. No matter how often it may be stretched or deflected, up to a certain point, the general belief is that it will come back to its original form every time the load is taken off. There are high authorities, however, who maintain that iron takes a permanent

set under even very moderate strains. If we are to understand that the set is exceedingly small, this may be true. The late Mr. Hodgkinson, for example, remarked, on the 381st page of his "Experimental Researches," that two cast-iron beams took each a permanent set with weights respectively equal to one fifty-seventh and one eightieth of the breaking weight. In a discussion at the Institution of Civil Engineers, a Mr. Dines mentioned that he had tested upwards of 8000 cast-iron girders for the late Thomas Cubitt, and that he found it hardly possible to apply a weight so small as not to produce a permanent set, one-twentieth of the breaking weight producing a perceptible set. In the experiments of the Iron Commission at Portsmouth, a bar of annealed wrought-iron 50 ft. long, was said to have taken a perceptible set with a weight of less than $1\frac{1}{4}$ tons per square inch. After this weight had been doubled, however, the set was still only perceptible; and notwithstanding that the elasticity of annealed iron is known to be inferior to that of unannealed bars, the whole set of the 50 ft. bar was but the $\frac{1}{250}$th part of one inch, after a strain of $8\frac{1}{3}$ tons per square inch had been borne; and the set was but the $\frac{1}{20}$th of an inch in 50 ft. after a strain of 11·9 tons per square inch. Mr. Edwin Clark has experimented on a wrought-iron bar 10 ft. long and 1 in. square. Under a strain of 3 tons per square inch, he gives a permanent set of nearly the $\frac{1}{4000}$th part of an inch in 10 ft. With 8 tons the permanent set is given as about the $\frac{1}{1280}$th of an inch in 10 ft., and it was not until a strain of 13 tons per square inch had been applied that a set of $\frac{1}{32}$in. in 10 ft. became apparent. With such exceedingly minute measurements, we may, perhaps, doubt if there was really any permanent set at all, with strains under 9 or 10 tons per square inch. An increase of temperature in the bar of perhaps a single degree, while the measurements were being made, would more than account for some of the reported sets, even under considerable strains. Thus, Mr. Edwin Clark gives the permanent set of his bar, after a strain of 8 tons per square inch, as the $\frac{1}{153846}$th part of its length, and this is almost exactly what the extension of the bar would have been had its temperature been raised but a single degree between the observations. Iron is heated in the very act of straining it, and a sudden breaking strain will generally leave the broken ends too hot to be handled.

Such a slight apparent extension might also have occurred while the shackles by which the bar was strained were coming to their bearings. But even if such a microscopic permanent set really existed, it is one of which no engineer would take the slightest serious notice as affecting the strength of the bar in which it was observed. With the means of measurement commonly employed by engineers, ordinary wrought iron is seldom permanently stretched until after it has borne strains of upwards of 8 tons per square inch. In seven experiments by Professor Barlow, on wrought-iron bars 10 ft. long, two of them retained their full elasticity under a strain of 11 tons per square inch, three bars bore 10 tons without injury, while one bore 9½ tons, and another, made from old furnace bars, did not retain its elasticity beyond a strain of 8¼ tons per square inch. All the links of Pesth Suspension Bridge, upwards of 5000 in number, and 12 ft. long from centre to centre, were tested without permanent set up to 9 tons per square inch, and those of Chelsea Suspension Bridge were tested, without permanent elongation, up to 13½ tons per square inch. Mr. Edwin Clark, from the results of his experiments, considers that the limit of elasticity of wrought iron is 12 tons per square inch, and this appears to have been adopted by him both for bars having a breaking weight of 24 tons, and for plates having a breaking weight of 20 tons. Every chain cable purchased by the Admiralty is tested up to 11·46 tons per square inch of the metal in each side of the link, the standard test being 630 lb. for each circular ⅛ in. of the diameter of the iron of which the cable is made, one half of this strain coming upon each side of the link. The iron of which the cables are made does not, as a rule, take any permanent set when strained to this amount, or to say 11½ tons per square inch. Mr. Howard has stated that the best iron begins to stretch permanently under about 10 tons per square inch in 10 ft. lengths, although he occasionally tests up to 15 tons or 16 tons per square inch, the breaking weight being from 26 to 28 tons. Mr. Mallet, about four years ago, presented to the Institution of Civil Engineers, the results of a valuable series of experiments on wrought-iron and puddled steel, from which it appeared that the elastic limit and the breaking strain under tension were, in the case of certain samples, as follow:

	Elastic limit.		Breaking weight.
Hammered slab or bar, 12 in. × 4 in......	15·312 tons	...	24·062 tons
Hammered bar..............................	14·219 „	...	22·969 „
Rolled slab or bar, 12 in. × 4 in	10·937 „	...	22·969 „
Rolled bar..................................	10·937 „	...	22·969 „
Fagotted forged slab, 4 ft. × 4 ft. × 1 ft...	8·750 „	...	18·594 „
Original fagot bars, Horsfall gun..........	12·031 „	...	21·875 „
Longitudinal cut, forged mass.............	9·844 „	...	19·688 „
„ „	10·937 „	...	17·900 „
Circumferential „	6·562 „	...	16·406 „
„ „	5·470 „	...	16·716 „
Transverse..................................	3·281 „	...	6·562 „
Charcoal rolled bar from borings from the Horsfall gun........................	5·470 „	...	22·321 „

Sir Marc Brunel made a number of experiments on Yorkshire iron, hammered to small dimensions, or from $\frac{5}{8}$ in. to $\frac{1}{2}$ in. square. A very high elastic limit was obtained, as follows:

Mean of ten bars, began to stretch with 22·2 tons per square inch, the mean breaking weight being 30·4 tons per square inch. With ten other bars the mean strain at which they began to stretch was 24·4 tons, the breaking strength being 32·3 tons per square inch. It is to be borne in mind, however, that these bars were reduced by the hammer only at the centre of their length, and that, therefore, the stretching could be observed upon but a very small part of their length. Mr. John A. Roebling, the engineer of the Niagara Suspension Bridge, has made experiments upon bars similarly drawn down to $\frac{3}{4}$ in. square at the centre, the breaking weight being 33 tons per square inch; these bars bore a strain of $20\frac{1}{2}$ tons per square inch without visibly stretching, and when no jar was given to the bars they would support this strain for a week. Upon any vibration, however, the bars immediately took a permanent set.

Under strains, however, considerably within the elastic limit, a gradual re-arrangement of the particles of the iron commences; and if the strain be continued sufficiently long permanent set will after a while take place. The late M. Vicat, whose work on limes and mortars is so well known, began as early as 1830 to investigate the

effect of continued strains on unannealed iron wire. He applied various strains to similar wires of a known breaking strength, and continued these strains from July, 1830, to October, 1833. One wire was strained to one-fourth its breaking weight, but beyond the elongation which at once took place no additional stretching occurred in thirty-three months. A second wire was strained to one-third of its breaking weight, and in thirty-three months it stretched at the rate of $2\frac{3}{4}$ parts in every 1000 parts of its length, this stretching being additional to that which took place as soon as the weight was applied, but which, of itself, was not sufficient to immediately produce any permanent set. Under a strain of one-half of the breaking weight another wire stretched rather more than 4 parts in every 1000 parts of its length. Under a strain of three-fourths of the breaking weight a fourth wire stretched, in thirty-three months, $6\frac{1}{8}$ parts to every 1000 parts of its length, and then broke, which circumstance terminated the experiments. M. Vicat's account of them appeared in the 54th volume of the second series of the *Annales de Chimie et Physique*. It is to be regretted that, in place of the constantly recurring experiments upon the breaking strength of iron, and which, as is already beginning to be understood, give us but a very partial knowledge of its available working properties, we have not a larger experimental acquaintance with the continued supporting power of iron, as afforded by experiments similar to M. Vicat's. Mr. Fairbairn, it is true, made an extensive series of experiments between the years 1837 and 1842, to ascertain how long bars of cast-iron would support weights equal to about nineteen-twentieths of their breaking weight. By taking care to prevent any vibration in or about the bars, several of them continued, for five years and upwards, to support nearly their full breaking weight. Their deflection steadily increased, however, during the whole time, and Mr. Fairbairn has stated that some of these bars afterwards broke with but one-twentieth of their original breaking weight. As bearing upon the last-mentioned circumstance, it may be remarked that M. Vicat, writing in 1833, observed that M. Henri, an engineer serving in Russia, had already shown that iron which had once withstood a great proof strain often broke some time afterwards under a much less strain. This fact must indeed have been known to practical men even earlier than in 1830.

Now, in employing iron in any structure where it is subjected to strain, we seek to keep within its limit of elasticity. Yet not only have we but a comparatively small number of recorded experiments to show us what this limit is, even under a single and temporary strain, but we have at least the result of M. Vicat's experiments to show us that we cannot depend upon anything like this limit under a long-continued strain. What experimental knowledge we have goes to show that the original elastic limit of iron is greater when hammered than when rolled, but we are unable to count with any degree of certainty upon the ultimate superiority of hammered iron, in this respect, after long-continued strain. As a rule, also—the abundant evidence of which it is not, perhaps, necessary to introduce into the present paper—all harsh, hard crystalline irons have a higher elastic limit, in proportion to their breaking strength, than soft, ductile, highly fibrous irons, like Swedish bar, for example. That is to say, the harder irons will bear a greater strain, before taking a permanent set, although, as we shall presently have occasion to inquire, it may not follow that they are really superior to other irons which are more readily stretched, and which, indeed, may have an even less breaking weight. What information we have goes to show that there is no settled relation between the elastic limit and the breaking weight of iron; the former is much more variable than the latter, and can hardly be expressed at all as an average result, ranging, as it does, from less than one-fourth to more than two-thirds of the breaking weight; or if the elastic limit be taken irrespective of the breaking weight, the instances already cited show that the former varies from $3\frac{1}{4}$ tons up to $24\frac{1}{2}$ tons per square inch in different qualities of iron, although the range in ordinary bar iron-and plate-iron is not nearly as great. Now, no engineer, in apportioning the strains in a structure, would think of working up to, or near to, the breaking strength of iron. His object is to keep within the elastic power of the material, not merely as ascertained by a single strain of a few minutes duration, but the continued elastic power of the iron as exerted through a very long series of years. We ought by this time to have hundreds of trustworthy experiments upon this point where there is now one. If the safe working strength of a metal is limited, as it would appear to be, by its measure of permanent per-

fect elasticity, we may say that we hardly know, even yet, what is the strength of the materials we are constantly dealing with, notwithstanding that not a year passes without some addition to our stock of knowledge of breaking weights.

While we are about considering the permanent injury which iron suffers when strained beyond its elastic limit, it is to be understood that iron may be strained for a short time almost up to the breaking point without in the least diminishing its strength under a breaking weight subsequently applied. Indeed, in gradually applying the breaking strain to any sample of iron, it is clear that it must have borne 10 tons before it is subjected to a strain of 15 tons, and that it must have borne 15 tons before it is strained to 20 tons, and so on. Not only is this the case, but after a bar of iron has been actually broken under a tensile strain, the two broken portions of the bar will almost always require a still higher strain to break them. The weakest spot in the bar will fail first, and although the breaking strain will at the same time permanently stretch the bar throughout its whole length, the iron on each side of the fracture will still have its original breaking strength. Professor Barlow's Treatise on the Strength of Materials contains the results of several experiments made at Woolwich Dockyard, as follows :

A bolt of Solly's patent iron was nicked at one place, and then broken by a strain of 24·7 tons per square inch. One of the pieces was then tried and broke at $29\frac{1}{4}$ tons. In the next experiment a bar of the same iron was first broken with $23\frac{1}{2}$ tons per square inch then again with $26\frac{1}{2}$ tons, a third time with $26\frac{1}{2}$ tons, and a fourth time with $25\frac{3}{4}$ tons.

Mr. Thomas Lloyd, engineer to the Admiralty, make a like series of experiments a few years ago, on ten bars of SC crown iron $1\frac{3}{8}$ in. in diameter, and $4\frac{1}{2}$ ft. long. The mean breaking weight at the first breakage was 23·94 tons per square inch. At the second breakage, with pieces 3ft. long, the mean strength was 25·86 tons per square inch. At the third breakage, with pieces 2 ft. long, 27·06 tons per square inch, and at the fourth breakage, with 15 in. lengths 29·2 tons per square inch. Mr. Lloyd's experiments have been held to show that iron was actually strengthened by stretching it; or, in other words, that by destroying the cohesion at one point, the co-

hesion was everywhere else increased. A more obvious explanation is that the bars first broke at the weakest part, then again at the next weakest part, and so on. A variation of from 23·94 tons to 2·92 tons in the strength of the same bar is undoubtedly large, the greater strength being 22 per cent. more than the lesser: a difference which appeared to exist in each of the ten bars tried. It is well known, however, that hardly any two bars of iron have exactly the same strength, and Mr. William Roberts, manager of Messrs. Brown, Lenox, and Co.'s extensive chain cable works at Millwall, has cut a 12 ft. bar of iron into 2 ft. lengths, and found on testing that there was a difference of strength of 20 per cent. between the strongest and the weakest of these pieces. In the experiments of the Railway Iron Commission upon the extension of cast-iron, the strength of Lowmoor cast bars was 7·325 tons per square inch at the first, and 8·152 tons at the second breaking. Blaenavon iron broke with 6·551 tons per square inch at the first, and 6·738 tons at the second breakage. Gartsherrie iron broke with 7·567 tons per square inch at the first, and 8·475 tons at the second breakage. Other cast-iron bars of a certain mixture broke with 6·6125 tons per square inch at the first, and 6·777 tons at the second breakage, the latter being at an unsound place. Upon these results the commissioners remarked that "it would appear that iron, repeatedly broken, becomes more tenacious than it was originally. This erroneous conclusion may be obviated by considering that it would be very difficult, if not impracticable, to obtain cast-iron bars perfectly sound and 50 ft. long. Fracture may be supposed to take place the first time at the largest defect, and subsequently at those smaller, until, finally, none remain." It is not intended, however, in the present paper, to entirely deny that the breaking strength of iron may be actually increased by being stretched when cold; and this point may be left as an open question. That iron is so strengthened derives some probability from the known fact that its strength is greatly increased by being drawn cold into wire, and also by cold rolling. When heated moderately, or to less than a dull red, and then stretched, iron is strengthened throughout. This treatment is known as thermo-tension, and in an extensive course of experiments made about twenty years ago by Professor Walter R. Johnson, for the United States Govern-

ment, a total gain of nearly 30 per cent. in strength and length, taken together, was estimated to have been obtained with a variety of irons. A bar of iron, having a strength of 60 tons, was heated to upwards of 500 deg., and then stretched by 6¼ per cent. of its length, when it acquired a strength of 72 tons. Captain Blakely has lately proposed the same treatment of iron, and his experiments, it is understood, corroborate those of Professor Johnson. All the links made for the four great pitch chains employed, with the steam-ship *Great Britain's* engines, for getting up the speed of the screw, were stretched ⅛ in. while at a dull red heat.

But from what has been said, it is not to be supposed that iron is not injured by excessive strains, notwithstanding that the metal strained may, when tried immediately afterwards, still retain its full breaking strength. The injury will appear when a subsequent working strain is long continued; and even without waiting for this, it will be found that the strained iron has been deprived of a large part, if not the whole, of its natural elasticity. In a paper of great value, read nearly seven years ago before the Royal Irish Academy, and afterwards published in a quarto volume entitled, "On the Physical Conditions involved in the Construction of Artillery," Mr. Robert Mallet has laid down a useful measure of the working and ultimate strength of iron. Poncelet had already employed co-efficients which indirectly expressed, not merely the elastic limit and breaking strength of iron, but the range also through which the force acted in each case in reaching these limits. Mr. Mallet has adapted these co-efficients to the English standard of mechanical work, to wit: "foot-pounds," and he represents the structural value of different materials, or of various qualities of the same material, in one case by the product of the elastic load in pounds into half the range in feet or parts of a foot through which it acts, and in the other case by the breaking weight in pounds multiplied also by half the range, in feet or parts of a foot through which it acts. Mr. Mallet employs Poncelet's co-efficients, as follows:—

Te = foot-pounds in reaching elastic limit of tension.
Tr = " to produce rupture by tension.
T^1_e = " in reaching elastic limit in compression.
T^1_r = " to produce crushing.

One half the weight into the whole extension, or, what is the same thing, the whole weight into half the extension, is adopted because the force gradually applied to break a bar must increase from nothing to the breaking weight. Upon Dr. Hooke's law, *ut tensio sic vis*, the weight of a grain will in some minute degree deflect or extend the heaviest bar of iron, and the deflection or extension will increase progressively, with the weight applied, up to rupture. Therefore, if a bar be stretched 1 ft. and then broken with a weight of 33,000 lb., the work done will be the mean of zero and 33,000 lb. into 1 ft., or 16,500 foot-pounds. This, as has been said, is the work done in the case of a gradually applied strain. If, however, the weight be applied without impact, but at the same time instantaneously, upon the bar, it will, so long as the limit of elasticity is not exceeded, and supposing the bar to have no inertia, produce twice its former deflection, and, therefore, twice the ultimate strain. For the weight, in falling through the distance of the deflection due to the load at rest, will acquire momentum sufficient to carry it through an additional distance equal to that of the static deflection. This may be best demonstrated experimentally with the aid of a spring balance. If, upon the pan of a balance sufficiently strong to weigh up to 40 lb., a weight of 15 lb. be placed, and this be then lifted to zero on the scale and there released, it will descend, momentarily, to nearly 30 lb. on the scale; and if there were no opposing resistances and the spring had no inertia, it would descend to exactly 30 lb. In the actual application of strains in practice, a weight is never thus applied, but a consideration of what would occur under such circumstances is sufficient to show how important it is that vibratory action be not overlooked in considering the strains on bridges. It is to be remembered that this action of suddenly applied loads is only manifested in the case of the application of weights, for if the strain be produced by the sudden admission of steam or any other practically imponderable body, no additional deflection will take place beyond that due to the pressure acting statically. If steam pressure acted in the same manner in this respect as a weight, the steam indicator would show nearly or quite double the pressure acting effectively within the cylinder of the engine.

It will not be attempted in the present paper to enter fully into

the application of the coefficients adopted by Mr. Mallet, for there are objections against, as well as reasons in favour of, their application. It is evident that *Tr* may be the same in two cases, in one of which a high breaking weight is exerted through a very short distance, and in the other of which a low breaking weight produces stretching through a correspondingly greater distance. But this coefficient does possess a value in taking account of the combined cohesive force and extensibility of iron, instead of the breaking strength alone. As Mr. Mallet justly observes, glass has a high cohesive force, and is, nevertheless, useless under strain, owing to its brittleness, while caoutchouc has great extensibility or toughness with but slight cohesion. The products, therefore, expressed by the coefficients in question do not afford a complete notion of the practical value of a given material unless the factors whereby these products are obtained are also given. The elastic limit of iron, however low, is not to be exceeded in practical use, whatever its range of elasticity may be; nor does it appear prudent to work into the neighbourhood of a high elastic limit when the elastic range is known to be small. It is not to be understood that the coefficients in question are intended to apply otherwise than in comparison of bars of equal length, else it would result that the measure of *Te* in a bar 50 ft. long, was one hundred times greater than that of a bolt 6 in. long, and of the same material and sectional area. For the purposes of the engineer a long bolt is not only no stronger than a short one, but, as it can be no stronger than its weakest part, it will follow that the average strength of 100 bolts 6 in. long, is likely to be greater than that of a single bolt of the same diameter and 50 ft. long. Every engineer must be aware of the importance of toughness in combination with cohesive strength in iron, but we need much more extensive and accurate information as to the former; and a consideration of Mr. Mallet's coefficients should lead to additional experiments being undertaken. It is but right to mention here that, in 1848, Mr. Homersham Cox contributed a paper to the *Civil Engineers' and Architects' Journal,* in which he represented the mechanical work, in foot-pounds, expended in deflecting a girder, the work being expressed by the product of half the weight into the range of deflection. Mr. Kirkaldy, proceeding upon an independent course of inquiry, but with the same

object as that pursued by Mr. Mallet, has lately published the results of a most important series of experiments, which are the first upon anything like an extensive scale, to take into account the combined cohesive force and extensibility of iron and steel. Mr. Kirkaldy experimented upon many hundreds of specimens, but he did not ascertain their limit of elasticity. He has given both the original dimensions and cross sectional area, and the dimensions and area after fracture, and he has also given the amount of elongation at fracture, although he did not ascertain the extension at the elastic limit. The reduction of diameter of a bar at the point of fracture serves to give a practical man a good idea of the quality of the iron, but it does not admit of an expression of the mechanical work done in producing fracture, as does the combined breaking weight and linear extension. In tearing a bar in two, also, we have to consider the permanent stretch communicated to all parts of the bar alike, and the additional stretch at and near the point of fracture. That part of the stretching which extends uniformly throughout the whole bar would, we may suppose, be exactly proportional to the length of the bar, while that part of the stretch which takes place close to the point of fracture would, we may also suppose, be a fixed quantity, whatever might be the length of the bar. Mr. Kirkaldy's specimens of iron and steel varied from 2·4 in. to 8·2 in. only in length, and with these the ultimate elongation at fracture varied from nearly nothing to 27 per cent. of the original length, whereas longer bars would have shown a proportionally less elongation. The samples which hardly elongated at all were of puddled steel ship plates. One sample, which bore 63,098 lb. per square inch of the original area, stretched before breaking but the $\frac{1}{20}$th part of an inch in a length of 7·6 in., or less than $\frac{7}{10}$ths of 1 per cent. of the length. Adopting Mr. Mallet's coefficient, the structural value of such a material would be almost nothing. In fact, Mr. Kirkaldy found the puddled steel plates throughout to have much less extensibility than cast-steel plates, while the former also were of very irregular breaking strength. The interest attached to the subject of puddled steel may possibly serve as a justification for mentioning this fact, although foreign to the object of the present paper, which has to deal with iron.

We have, up to this point, considered the elastic limit or perma-

nent supporting power of iron under a single application of strain, extending in some cases over a few minutes, and in others over some months. We have very little experimental knowledge as to the extent to which we may approach the elastic limit under continued strains, but there is a probability that a continuous strain, much exceeding one half of the elastic limit, would be attended with a gradually progressive permanent set. At the same time, with good iron possessing a fair amount of extensibility, a single temporary strain, producing a slight or even considerable permanent set, may not diminish materially the strength expressed by Mr. Mallet's coefficient *Tr*, and the iron may still hold up indefinitely with an ordinary load considerably within the limit of elasticity. But apart from the consideration of the time through which a strain acts, we have also that of the frequency with which the load is applied and removed. Mr. Fairbairn loaded cast iron bars to upwards of nine-tenths of their breaking weight, and this load remained on for from five to seven years before the bars broke down; but with a repeated application and removal of load, the Iron Commission experiments showed that iron bars, when repeatedly deflected, by impact, through one half the distance due to their breaking weight, broke down after from 130 to 4000 applications of the strain. Mr. Fairbairn communicated some of the results of an important series of experiments to the British Association at the Manchester meeting, 1861, from which it appeared that a large model of a wrought-iron plate girder withstood without injury 1,000,000 applications of a load equal to one-fourth its breaking weight, and afterwards 5175 applications of one-half its breaking weight, when it broke down. The model was then repaired, and 25,900 applications of two-fifths its breaking weight, and afterwards nearly 3,000,000 applications of one-third its breaking weight, were made, it was said, without injury, although neither the deflexions nor the permanent set were given. We know that iron alters its form, temporarily, during the application of very moderate strains, the elasticity of good iron being generally observable with strains of one ton per square inch. And we know that its form is permanently changed both immediately on exceeding the limit of elasticity, and gradually under strains nearly approaching that limit. There clearly must be a re-arrangement of the

particles of iron always going on where the strain is great; and as we know that when even a more moderate strain is eased off the iron tends to resume its original form, it appears incontestible that final injury must result under what may appear moderate although irregular loads, say one-third or even rather less of the breaking weight. Iron, it is still to be remembered, has not been employed long enough for purposes of construction to enable us to compare its endurance with masonry, of which there are abundant examples still perfect after many hundred years. At the same time it must not be forgotten that, while we can never know the absolute strength of a bar of iron without destroying it under strain, neither can we always infer its strength from its deflection, or apparent range of elasticity. For we are not yet secure against flaws or those other faults of molecular structure which Mr. Mallet so well describes as "planes of weakness." A bar of iron may have a general strength of 22 tons per square inch, except at a single point in its length, where for an almost inappreciable linear distance, the strength may not exceed 10 tons. If the bar be broken this fault will be detected, but hardly, if at all, otherwise. For under a strain of even 8 tons or 9 tons, the extension at the precise point of weakness would be so slight as to be quite overlooked in a general observation of the total deflection or extension.

In this fact, which was mentioned thirty years ago by M. Vicat, and in the known effects of suddenly applied strains, vibration, impact, &c., we have abundant reason for extreme caution in the use of iron for permanent structures of any kind. It can hardly be said that the Board of Trade limit of 5 tons, arbitrary as it is, is too moderate, although there should be some account taken of the known quality of the iron to which this strain is to be applied; and it may not be necessary to adopt an unlikely load as that which is to produce the maximum strain of 5 tons. The recent discussion as to the security of Chelsea Bridge affords much suggestive material upon these points.

The application of iron to bridges, especially to those of large span, necessarily requires the most careful consideration in apportioning the strains, since every pound of metal not brought into effective action is so much dead weight or useless load—being not only mis-

applied of itself, but requiring additional material to support it. In considering the strains upon iron, therefore, reference has been more particularly made in the present paper to its employment in bridges, but in the case of boilers, iron ships, cranes, ordnance, railway bars, warehouse girders and columns, roofs, engine beams, and in many other applications, the most careful distribution of material and adjustment of strains is of very great importance. Iron is, perhaps, more severely strained in steam boilers than in other structures. In the case of locomotive engines, there is a disposition to employ still larger boilers and to carry still greater pressures. With 50-inch boilers, formed of $\frac{7}{16}$ inch plates double riveted, and carrying, as is now not unusual, from 130 lb. to 150 lb. pressure, there is at the higher limit a circumferential strain of $5\frac{1}{2}$ tons per square inch at the joints and a longitudinal strain of nearly 2 tons per square inch along the whole length of the boiler: the resulting strain at the joints being nearly 6 tons per square inch. This strain is constantly maintained with plates ranging from 21 tons to 24 tons in strength and under all the contingencies of corrosion, incessant vibration, and occasional sudden exaltations of pressure due to the instantaneous production of steam upon overheated tubes or plates. In many cases we have 4 ft. boilers with $\frac{3}{8}$-inch plates, single riveted and worked at 120 lb. corresponding to a strain of at least $6\frac{1}{2}$ tons per square inch at the joints of the boiler when new: the circumferential and longitudinal strains being both taken here into account. Put under this strain when new, many locomotive boilers are worked in all for from ten to twenty years, and often from three to seven years without any internal examination of the plates. It is not remarkable, therefore, that explosions are becoming so frequent.

We may regard with much hope the increasing use of steel in large masses, as produced by Krupp and by Mr. Bessemer, and others whose discoveries have already effected a great economy in the production of that material. Although a departure from the subject of the present paper it is interesting also to refer to the introduction of phosphorised copper, as now produced by the Birmingham and other coppermasters. It was announced, about three years ago, as a new discovery by Mr. Abel, chemist to the War department, that the addition of from 2 to 4 per cent. of phosphorus to copper greatly

increased its density and strength. There is no doubt of the large advantage of this combination, although it was discovered in the last century, and made publicly known sixty years ago. A French chemist, M. Sage, contributed a paper upon this subject to the *Journal de Physique*, and which was translated into the 20th volume of the *Philosophical Magazine*, for 1805. By combining the maximum quantity of phosphorus with copper, the latter acquired the hardness, grain and colour of steel, and although M. Sage had already kept the compound for fifteen years it had suffered no change from exposure to the air. It was easily turned and took a fine polish. It may yet be found that copper thus treated is the best material for many of the purposes of the mechanical engineer.

In conclusion, it must be regretted, if not indeed deplored, that our exact knowledge of the permanent supporting power of iron is so limited. The present paper, it is feared, will hardly fulfil its title, which may have led to the expectation that some definite relation would be assigned between the safe load and the ultimate strength of iron. Even at the risk, however, of leaving the subject as the author found it, he indulges the hope that the considerations which have been touched upon may induce closer observation and inquiry into the real working properties of iron, as distinguished from its ultimate strength. The question involved in this subject does not appear to be one which admits of solution by a reference to any known natural law, nor does it appear that it can be disposed of by mathematical investigation, but our only safe conclusions must be derived from the results of protracted, varied and repeated experiment. In the meantime common prudence requires that we ascertain, with as much accuracy as possible, the elastic powers of every given kind of iron proposed for permanent works, and that we then so proportion the parts that the working strain shall never much exceed one-half the elastic limit, a ratio which will correspond, according to the variable quality of iron, with a load of from one-eighth to one-fourth its breaking strength.

DISCUSSION.

Mr. W. Roberts agreed with the Chairman that the paper just read was of a most valuable and interesting character. It introduced many points which were deserving great consideration. He referred

more particularly to one, namely, the point at which the elasticity of iron terminates and where it commences. He had no doubt that where permanent set had been noticed at a very low strain, that it had occurred from an alteration in the temperature. He had observed that, in testing samples of iron, even a cloud passing upon a fine day would cause the iron to go back when it had apparently stretched, which certainly would account for these very low strains. During the whole of the experiments he had conducted, extending over a period of some years, he had never found permanent set in good iron under 9 to 10 tons to the inch. It was true the longest bars had been under 10 feet. He had not only found that iron bars, after a strain had been rapidly put upon them up to 9 tons, would go back, but when a second strain had been put upon them equally rapidly, and when apparently permanent extension had been established, the iron bars after a few hours go back again to their original length. He did not know that any gentleman present, who had been trying experiments had observed that, after good soft iron had attained a maximum strain, it had gone back, and ultimately broke at from 15 to 20 per cent. less than it had borne. He thought that was a point worthy of attention.

In reply to the Chairman, who inquired whether Mr. Roberts had tested any chains that had been a long time in use,

Mr. Roberts stated he had tested cables that had been 25 or 30 years in service, and found that they stood 20 per cent. more than the Admiralty proof. He had also found a chain (two inch) that had been used constantly for testing purposes, ultimately break at a much less strain than had been put upon them at different times. He had had some bars in their testing machine, exactly the same size as the back guys in Mr. Humphrey's sheers, and many of those bars maintained their elasticity up to 15 tons to the inch, and this was about the strain that *good* iron would bear without permanent set.

Mr. Parsey said there were many collateral circumstances that had to be considered in connexion with the elasticity of iron. For instance, something must be known of the amount of inspection and supervision to be exercised during the manufacture of the work, before the proper limit could be determined. He had had opportunities of testing iron used for different purposes, and of applying a very accurate machine to test the extension under strain. The rule

adopted had been to first put a pressure of 10 tons to the square inch on the bar under strain, and he thought he could say that in good iron he had never found permanent set under 10 tons. He could point to some hundreds of experiments. In some cases there was an extension, or perhaps permanent set, with less than 12 tons per inch, but frequently there was some defect observed after fracture in the bar. It was his opinion that the apparent permanent set was due to the flaw. He had never in the whole course of the experiments found iron attain the high strains that many manufacturers talked about; some, for instance, said that their bars would bear a strain of 30 tons to the inch, but he had seldom found small bars bear more than 26½ tons, and when the bars had a large sectional area of five or six inches, they will hardly bear 24 tons. The experiments he had referred to had induced him to come to the conclusion that good iron will bear a strain of 10 tons to the inch without injury. He thought if they adopted the coefficient of 5 tons to the inch, and the work was properly constructed, and no bars were allowed to be put into structures with flaws or defects, those structures would be perfectly safe. He had tested pieces of steel iron reputed to bear a strain of 40 tons to the inch, but had found them break under a pressure of 27 tons. He had also tested some pieces of Bessemer steel plates which bore a strain of over 50 tons to the inch; there was no doubt that that sort of stuff in large spans would be of great advantage. Referring to Chelsea Bridge, it had been stated that the bars would bear a strain of 30 tons to the inch, all he could say was, that he had had opportunities of breaking many similar bars, and had never found them to come up to 24 tons, the average not being above 22 tons—many had broken at 18 or 19 tons—but he must mention that the latter were made of single hammered scrap, and had been rejected. Therefore, in estimating the strain of those links they must not reckon to have 30 tons to the inch, for he believed it was perfectly fabulous, and the slightest defect in any portion of the bar would reduce the strength considerably. They must also consider the size of the bar and the diameter of the pin, and to see if they bore the proper proportions.

The Chairman inquired if Mr. Parsey had ascertained the elasticity of Messrs. Howard and Ravenhill's links?

Mr. Parsey said it was about 12 tons to the inch. He had some-

times found them come up to $13\frac{1}{2}$ tons, but not as a rule. Generally the bars were very good indeed, but still not so good as had been talked about.

The Chairman inquired how Mr. Parsey allowed for any change of temperature?

Mr. Parsey said the instrument was screwed on to the iron, so there was no chance of a variation of temperature affecting them. He sometimes found there was an apparent set, but it went back. That he thought had been observed by others, namely, that when the strain was put on and drawn off suddenly, the iron did not recover itself instantaneously. It was very frequently thought there was permanent set when it was due to a flaw in the bar.

The Chairman inquired whether the measuring-rod was made of iron or wood?

Mr. Parsey replied that the rod was made of gun metal.

Mr. W. Roberts remarked that he always used yellow deal for his measuring-rods.

Mr. C. J. Light said that the relation of the safe load to the ultimate strength of iron must vary considerably, according to the class of structure for which the iron was to be employed. For instance, if there was a dead permanent load that did not vary at all, it was clear that the relation between that load and the ultimate strain must be very different from that in the case of a strain on a railway bridge.

Mr. F. Young stated that though Mr. Parsey thought no bars would bear a strain of 30 tons to the inch, such a strain was mentioned by Mr. Kirkaldy in his book on the strength of iron. He had himself seen some Shelton bars bear a strain of 30 tons to the inch before they broke. It was generally assumed that cast-iron should not be employed where much tension would result; but he knew a case where a cast-iron boiler had been in use for forty years with a pressure of 40 lb. to the inch. He referred to the mills of Messrs. Barrett at Ratcliff, where there was a cast-iron boiler still in use, which lately had had a wrought-iron patch put upon it. He might mention also the case of an American boiler made of $\frac{1}{4}$-in. plate-iron, which was worked at a pressure of 200 lb. to the square inch. It showed that either the plate or workmanship must have been very good, far better than was generally found in this country. He quite

agreed with Mr. Roberts that measuring-rods for testing elongation, should be made of wood, and not of metal.

Mr. W. Roberts had never seen iron that stood 30 tons to the inch in any boiler plate. He had seen steel stand that pressure. He had just completed a boiler from an idea for which he was indebted to Mr. Colburn, and its working results he would be happy to give to the society upon a future occasion.

The Chairman asked Mr. Roberts the breaking weight of the iron used for his cables?

Mr. Roberts replied that the breaking weight of the best Trinity iron was about 27 to 27½ tons to the square inch, and their ordinary Admiralty iron about 26¾ tons.

Mr. Parsey thought that there was no doubt the first working might leave some defects in it, which might be improved by working over again, the parts would be better welded; but he thought after the second working, the strength of the iron would be reduced instead of advanced. He had had opportunities of breaking iron made by several makers, but he had never found the strength very much greater in the very high brands. The higher qualities were softer and more ductile, and certainly better for some purposes; but he did not think the ultimate strength was greater. He should not take the result which Mr. Young had cited as being correct, without he (Mr. Parsey), had first overhauled the whole of the apparatus; for then very different conclusions might be arrived at. A short time since, at the Institution of Civil Engineers, the subject of the lasting qualities of iron structures was discussed, and the general opinion was that iron structures would last for a great many years. In passing over Hungerford Bridge a short time since, he observed the heads of the tension-bars (which were being taken down) where they had been in contact—they were perfectly sound, which confirmed the opinion that the structure would have lasted many years. Therefore, if constructed within the elastic limit of the iron, it was to be supposed, if the iron was in close contact and the parts painted outside, that the bridge would last perhaps for five hundred years.

Mr. Perry F. Nursey was of opinion that the reduction of the area of pins, or weakening of joints by rust was not a matter for very serious consideration. Deterioration of metal by oxidation was reduced to a minimum in iron structures under disturbance, in some

cases oxidation did not occur at all. This was particularly observable on lines of railways where there were rails under traffic and others in disuse, the former would be found to be free from rust, while the latter would be coated with it. He believed the absence of oxidation to be due partly to the mechanical and partly to the electrical action arising from passing trains, and was not confined to the points of contact, but common to all parts of the rail and its iron fastenings.

Mr. Colburn expressed his best thanks for the very favourable way in which his paper had been received. He felt that he had not arrived at any conclusion upon the subject, because, really there was but little information upon which to base a conclusion. From what had been recorded, there was no question that the breaking strength of iron was very variable, and that the elastic limit was very much more variable. The elastic limit seemed to be the limit of the useful strength of the iron. In seeking for information upon these points, it was really surprising how very little information had been recorded, and how few of the opinions of practical men got into print, which was much to be regretted, seeing that it was the only data that could guide engineers when they had to work close. In locomotive boilers there was a great strain, and proportionate allowance should be made. In cases of stationary loads there could be no doubt that the ultimate strain could be worked up to a closer point, than in a locomotive boiler. In cases of water-tanks supported by beams, the limit of the breaking strain could be much nearer approached than in cases where there was vibration: that was shown by Mr. Fairbairn's experiments. His (Mr. Colburn's) object in bringing all these considerations together was to induce a greater attention to the subject, more especially to the elastic limit of iron. He hoped that experiments would be much more frequently made and recorded. With regard to the case mentioned by Mr. Young, of the strength attained by an American boiler plate, he (Mr. Colburn) might mention that the plate referred to was rolled from cold blast charcoal iron. The experiment took place upon the Baltimore and Ohio Railway: the weight actually applied in several cases was over 2 tons to one sixteenth of the square inch. There was no doubt the result was attained; but all these higher strains must be looked upon as exceptional.

The Chairman said that Mr. Colburn's paper, and the remarks made upon it, had proved how very difficult it was to arrive at any satisfactory conclusion, either as to the amount of breaking strain of iron, without breaking it, or the elastic limit of iron without stretching it beyond that limit. A bar of iron might have a small flaw which could not be seen till the bar was broken, and therefore could not be measured in measuring the amount of elasticity. A great point would be gained if these flaws could be detected in testing. Mr. Parsey said he used a metal rod, or guage, for measuring the amount of elasticity in Messrs. Howard and Ravenhill's links; now, unless the same temperature was given to the measuring-rod as was given to the bar by the work done in stretching it, the amount of elasticity would not be measured correctly; and, again, the bar under test would appear to have a permanent set, unless it was allowed to arrive at the same temperature as when measured before testing. It had been stated during the evening that in testing bars up to, say, 10 tons, there was an extension, after the strain was taken off the bar, which gradually disappeared in one or two hours; this apparent permanent set was due only to the heat generated in stretching the bar, and as soon as the bar arrived at the same temperature as before testing, its length would be the same. If iron could be got for use that would stand 28 or 30 tons per square inch, instead of iron that broke at 18 or 20 tons per square inch, we certainly ought not to be bound not to exceed a strain of 5 tons per square inch, as the greatest strain. If a load giving 5 tons strain is good for 20-ton iron, certainly 7½ tons strain is equally good for 30-ton iron; therefore, instead of being bound, as was the case, not to exceed 5 tons per square inch for all kinds of iron, we were allowed to take ¼th of the breaking weight of whatever iron we used, we should be under a far more sensible restriction. On the continent they had the same restriction as here, always one strain for all kind of iron, no matter what the strength was. From what had been stated during the evening, it certainly impressed upon us the necessity of testing every part of an iron structure before use. The Chairman concluded by stating that the discussion upon Mr. Colburn's interesting paper would be resumed by Mr. Ordish at the next meeting.

April 6, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON THE RELATION BETWEEN THE SAFE LOAD AND THE ULTIMATE STRENGTH OF IRON.

By ZERAH COLBURN.

ADJOURNED DISCUSSION.

Mr. R. M. ORDISH said the paper read by Mr. Colburn to the Society at their last meeting contained a very valuable abstract of experiments, facts, and opinions on this very important subject; but the main question was still left open to form deductions from the data supplied. From those and other data engineers would arrive at very different conclusions, as recently evidenced by the investigations, as to the strength of the Chelsea Suspension Bridge, the engineers' reports and opinions on the question varying from 5 to 8 tons per sectional inch as the proper strain to be put on the main chains of this bridge, when loaded with a distributed load of 80lb. per superficial foot. For the purpose of arriving at a result as to the proper quantity of metal to be used in the construction of wrought-iron bridges or other load bearers, he took the requirements of the Board of Trade as regards Railway Bridges; viz.—that the tensile strain should not exceed 5 tons per square inch of section. The strain appeared to be reasonable and proper, and consistent with economical structures, and provision for the safety of the public. For several years past railway bridges had been made according to this requirement, and in most instances the quality of the iron used had been of mean breaking weight of 20 tons per sectional inch; therefore engineers generally, and the Board of Trade, were content with such structures, the iron being of a mean breaking weight of 20 tons per inch, the edges of the plates for the flanges, &c., being sheared, and the holes in plates, angle and T irons being punched. It was generally admitted on the authority of Mr. Fairbairn, that more than 20 per cent. of the strength of the iron was lost by punching,

such loss being distinct from that of the metal actually punched out. It was also generally admitted that the breaking strain per square inch of wrought-iron bars was 20 per cent. more than wrought-iron plates, the statement being likewise in accordance with the experiments made by Mr. Kirkaldy on the strength of iron. Therefore many railway bridges when riveted up and in place, were composed of iron of an effective breaking weight of 16 tons per sectional inch. Should a bridge be constructed of the same quality of iron as assumed above, but with the holes for the rivets drilled instead of punched, there was no doubt that the bridge would be stronger, and in the absence of experiments on the loss by drilling, he would assume that the iron would be of an effective breaking weight of 18 tons per inch. Therefore by drilling the hole a stronger bridge was obtained, but at an increased cost, the weight of metal remaining the same. The conclusion sought to be arrived at and proved, was that by employing a less quantity of superior qualities of iron, and better workmanship in bridge building, works were obtained of greater stability than the majority of railway-bridge constructions which were generally approved of, and were no doubt quite sufficient for their purpose. Instead of employing plates and angle iron riveted together for forming the tensile members of railway-bridge girders, if bars with enlarged ends were used similar to the links used in suspension bridges, it was found from the data previously summarised that instead of requiring an amount of metal dependent on the strain of 5 tons per inch, a strain of 8 tons per inch could be calculated on for determining the necessary quantity of material required, so as to give the same result. Assuming that the tensile strain or the bottom flange of a girder was 250 tons, it would require 50 inches effective sectional area according to the 5 tons per inch requirement of the Board of Trade. Comparing the required sectional area of iron of the punched and riveted form of construction against bar-iron with enlarged ends, and the pin-holes drilled, there was first a loss of 20 per cent. by punching, thus reducing the 50 inch area of assumed good iron to 40, and a further loss, or difference, between plate-iron and bar-iron of 20 per cent., reduced the 40 inches sectional area to 32 inches sectional area, which with a strain of 250 tons gave very nearly 8 tons per sectional inch, when the iron was assumed to be of equal quality to bar-iron.

Mr. W. T. Carrington stated, according to Mr. Fairbairn's experiments, more than 20 per cent. of the strength of the iron was destroyed by punching. A loss quite distinct from that of the metal punched out. That being the case, and taking plates—as Mr. Colburn said we sometimes did—of a breaking strain of only 18 tons per square inch before punching, we should have iron of a breaking strength of only 14·4 tons after being punched. The Board of Trade said that 5 tons per square inch should be the strain allowed in the case of wrought-iron bridges—thus allowing the working strain to be as much as one third of the breaking strain. Now, according to M. Vicat's experiment, iron being loaded to ⅓rd of its breaking weight stretched in 33 months at the rate of 2¾ parts in every 1000 parts of its length, in addition to that which took place as soon as the weight was applied. Therefore in the case of 18-ton iron in plate bridges, holes being punched, 5 tons was too much to be taken as the safe load if that strain remained on the bridge for a long time, it was therefore fortunate for the bridge, that ⅓rd the breaking strain was in action but a very short time, for if that permanent stretching continued to go on, as was to be inferred from M. Vicat, it was natural to conclude that the iron would break in time with ⅓rd its breaking strain. Such strain, according to M. Vicat, was too much to be taken as a safe continuous strain; but Mr. Fairbairn had said that 3,000,000 applications of ⅓rd of the breaking strain did not injure a large model of a wrought-iron plate girder. If both the above experiments were correct, they led to the belief that the experiments were made with different qualities of iron, and that the proportion of the safe strain varied considerably with various qualities of iron; such, no doubt, was the case, for the colder iron was when rolled, the greater strain it would bear without permanent set. Again, rolled bars of small section would bear a greater strain per inch, than the same kind of iron in bars of large sectional area. In fact, the closer the fibres of the iron were brought together, whether by the rolls or the hammer, the greater was the strain per square inch, required to give a permanent set.

In testing iron it had been noticed by Mr. Roberts, Mr. Parsey, Mr. Light, and others, that with a strain close upon the elastic limit, there had been an extension which gradually returned—some-

times in two hours, sometimes more—this extension could not be permanent set, or the bar would never have returned to its original length, and it could not be elasticity only, for if it was the bar would have returned to its original length the instant it was relieved of the strain. The most probable cause of such extension was the heat generated by the work done in stretching, and after the bar was relieved of its strain it would take some time for the heat to disperse, that time depending upon the mass of the bar, &c., besides depending upon the state of the atmosphere. If the measuring bar was made of the same material, and could have precisely the same heat given to it as the testing bar, then the extension spoken of would not be noticed, and for all practical purposes it ought not to be noticed, for it had nothing to do with the strength of the iron, it being caused simply by the heat generated by the work done. It appeared to him that a deal measuring bar would be the best in all cases of testing, or measuring, the extensions from strains, because its length was nearly always perceptibly the same, it varied but little; any bar of metal, no matter what kind of metal, would naturally vary, sometimes, considerably in length—the heat of the hand, a ray of light, a current of air, &c., was quite sufficient cause. Mr. Roberts said, at the last meeting, that he had seen bars shrink when a cloud had been passing over. He (Mr. Carrington) supposed the case of testing a bar, and while making the first measurement before putting the strain on it was cloudy, and while putting on the strain, taking it off, and making the last measurement, the sun shone, what would be the effect? It would be that the bar would appear to have a permanent set, no matter how small a strain had been put on; of course the extension would be evident if no strain at all had been put on. He mentioned this to show how very carefully testing experiments should be carried out before the amount of permanent set should be given for various strains.

If M. Vicat's experiments were correct, it might be supposed that $9\frac{1}{3}$rd tons strain applied for an instant, and 7 tons for, say, five years, on a wrought-iron bar one inch square, and in each case no permanent set had been given, and the elastic limit had been attained, then there would be a difference of $\frac{1}{4}$th of the elastic limit of the $9\frac{1}{3}$rd tons (that $\frac{1}{4}$th very nearly agreed with M. Vicat's experiments);

it could be proved by experiment if that difference was the same in all kinds of iron, and what reason could there be why it should not be the same? For take 6 tons instant strain of the elastic limit, and it was evident but little could be taken on either side of the $\frac{1}{4}$th of the 6 as the difference. If, therefore, it was correct to assume that there was always a proportionate difference, and that proportion once decided, then take the elastic limit strain of an instant load, deduct the above-mentioned difference, and the result would be the elastic limit of a permanent load, which would seem to be a strain that could always be safely worked to. It was such strain that might be called the "elastic limit" of iron. From what had been said there were two elastic limits: 1st, the elastic limit of instant load; 2nd, elastic limit of permanent load. The second had still to be decided by experiment, which must be very extensive and most carefully conducted. In the case of M. Vicat's experiments, the most probable cause of the gradual extension, or permanent set, was the action of heat, or variation in temperature, for if a wire was made to support one third of its breaking weight, heated, and then suddenly cooled, it was very certain that the wire in suddenly contracting must overcome the inertia of the hanging mass, and to do this it must exert a greater strain, which strain is over the elastic limit of the instant load, and would therefore give the permanent set. The more sudden the contraction the greater would be the extra strain, and therefore permanent extension. He thought M. Vicat did not attempt to keep his wires, while under experiment, at the same temperature for thirty-three months. If he had done so, it was most probable there would have been no gradual permanent extension of the wires from the permanent loads. In making the experiments required to decide the difference between the elastic limits of the instant load and the permanent load, great care would have to be taken that the iron should be subject to all the variations of temperature it would be subject to, as in the case of a bridge, otherwise the proper difference to be made use of would not be got. There was still that great difficulty of finding any flaw of infinitely small length. It was quite out of the question to expect that it could be measured, and if he could not find it by inspection and the elastic limit test, he would give it up as hopeless, and trust to providing against such small weaknesses by the number of parts

side by side, so that the chances would be always in favour of never having more than one flaw in the section of, say half a dozen pieces. When this provision could be made, no particular anxiety need be felt on the subject. In conclusion, Mr. Colburn thought his paper would hardly fulfil its title. He (Mr. Carrington) was sure no one could expect him to come nearer fulfilling it; if he could have given the data for arriving at the elastic limit of all kinds of iron, *i.e.* the elastic limit of iron strained for, say, five years or more, then he certainly would have conferred a great boon on all engineers; but if he had not given that data, he had shown very clearly how much it was required, and that it could be acquired by experiment, that was certainly the next thing to giving the data. He was sure many had no idea of the very great difficulty of arriving at the *useful* strength of iron, until Mr. Colburn read his most valuable paper.

Mr. Parsey said he thought he mentioned at the last meeting that between ten or twelve tons to the inch in well-manufactured iron did not produce permanent set, and that in a great number of instances, where there was an apparent permanent set, it was owing to a flaw in the bar; that would sometimes make it appear that there was a permanent set, when it was only an extension in the defective part. It was necessary to be very careful in estimating the assumed loads. He thought five tons to the square inch might be taken as a general rule, for it was only a proper measure of caution to provide for defect in the iron, and deterioration of strength by punching and oxidation. As regards the bars used in the construction of Chelsea Bridge, a statement had gone forth to the effect that they would bear a tensile strain of thirty tons to the square inch; now that appeared to him erroneous, as he had broken many similar bars under twenty-four tons to the inch; he thought they should be very careful in giving iron a very high working strain, unless they were perfectly sure that the bars would be perfect, and thoroughly inspected during manufacture.

Mr. C. J. Light said that, with the permission of the meeting, he would read the results of experiments on the strength of pieces of test iron that had been reduced to the square inch of section at one point only by two drilled holes, as compared with other pieces from

the same plates or bars cut to a parallel width for a length of twelve inches, as usual for trying extension. The results, in tons per square inch, in four pieces of plate iron, were as follows:

Cut parallel..................	21·0	24·21	24·1	22·5
Drilled......................	22·5	25·3	24·1	23·02
Difference per cent.........	$5\frac{1}{4}$	4	7	$2\frac{1}{2}$

Four pieces of T iron gave the following results:

Cut parallel..................	22·15	23·51	22·76	23·20
Drilled......................	23·14	25·6	25.98	24·58
Difference per cent.........	$4\frac{1}{2}$	$8\frac{3}{4}$	14	4

He explained the above differences on the supposition that in the parallel piece there was sure to be one point much weaker than the rest at which it would break, but the chances were greatly against the drilled hole coming at such a weak place.

There was also one point in connexion with plate-testing, which he had remarked and confirmed by several experiments, viz.—that in thin plates as $\frac{1}{4}$th or $\frac{5}{16}$ths, if the piece to be tested was fixed in the usual way by bolts or pins passing through its ends, the width of the reduced part in the centre should not much exceed the diameter of the pin, or an unfair strain would be put on the iron, the centre being strained to failing before the edges were able to aid it.

He then quoted some high tests of angle and bar iron from Rhymney Iron Works.

A piece of	$4 \times 4 \times \frac{3}{8}$	angle broke at	27·19	tons per inch.
,,	$5 \times 3 \times \frac{1}{2}$	,,	25·96	,,
,,	$7 \times 4 \times \frac{5}{8}$	,,	27·47	,,
,,	$9 \times \frac{3}{4}$	bar bore	28·2	,,

As these results were obtained not from prepared samples, but from the bulk of a large delivery, he thought them worth recording.

He had also obtained, within the last few days, some particulars with respect to some five-inch links lately tested at Howard and Ravenhill's works. The results were tolerably satisfactory, because

two of them came up nearly to 24 tons, and the extensions and permanent sets on a five-feet length were given at frequent intervals. The results were as follow:

LINKS 5 × $\frac{3}{4}$ IN BODY.

No. 1.

Tons per inch......	8	10	11	12	13	14	15	20	21·06
Extension...........	·05	·05	·06	·06	·07	·07	·67	3·82	broke.
Permanent set......	·00	·00	·00	·00	·07	·00	·60	3·69	9·62

No. 2.

Tons per inch.....	8	10	11	12	13	14	15	20	23·73
Extension.........	·04	·05	·05	·06	·075	·08	—	3·60	broke.
Permanent set....	·00	·00	·00	·00	·00	·01	—	—	11·0

LINKS 5 × $\frac{1}{2}$ IN BODY.

No. 3.

Tons per inch.	8	10	11	12	13	14	15	16	20	23
Extension......	·04	·05	·06	·06	·07	·07	·08	·25	3·16	broke.
Permanent set.	·00	·00	·00	·00	·00	·00	·00	·17	3·08	6·75

No. 4.

Tons per inch.	8	10	11	12	13	14	15	20	23·92
Extension......	·04	·05	·05	·06	·06	·08	0·9	1·79	broke.
Permanent set	·00	·00	·00	·00	·00	·00	·015	1·70	10·75

Mr. E. RILEY thought it would be satisfactory if Mr. Light would give some information as to the process of manufacture that the iron had undergone, and the materials from which it was made, as he understood Mr. Light to say it was common iron.

Mr. C. J. Light stated that in the experiments he had given, the iron was cut and tested in the ordinary course of inspection.

Mr. E. Riley said the iron appeared to have borne an extraordinary strain, and he thought it would be important to know the date at which the iron was manufactured, as he was aware that large quantities of Spanish ore, containing much manganese, had been used at Rhymney, and he thought that the presence of this metal in an iron ore had a most beneficial effect on the quality of the iron, and might probably account for its unusual strength.

Mr. C. J. Light replied that the experiments on the Rhymney iron were made in September and November, 1861, and the iron was supplied three or four months previously. It was hardly an exceptional case, as several other pieces bore almost equally high strains.

Mr. E. Riley inquired whether the iron mentioned was angle iron, and what means were employed to determine the permanent set?

Mr. C. J. Light stated that the piece that bore twenty-eight tons was bar iron, and the others were angle iron. The permanent set and extension were ascertained by means of a fine scribe.

Mr. Olrick wished to know if there had been any experiments with Bessemer's homogeneous metal. He (Mr. Olrick) called it metal because it was better than iron, and not exactly steel. He would also ask if any extensive experiments had been made with respect to the breaking or tensile strain of that metal? Where the breaking strain had been ascertained as well as the limit of elasticity, in the same kind of iron and under similar circumstances, he would ask whether it would not generally apply that a three-fifth part of the breaking weight would be a rough and ready rule for the limit of elasticity?

Mr. Roberts would make one remark with regard to punched plates. He believed that Mr. Fairburn and others contended that punched plates were more liable to contain flaws or defects than drilled plates. He (Mr. Roberts) agreed with them to a certain extent. In bridge building, boiler-making, &c., the joints were broken by one plate having its butt in the middle of the one above or below it; by this means one plate supported the other. When the edges of the plates were punched, he had no doubt there was as much as 20 per cent. difference up to a certain point in the plate. But he did not

think that could interfere very much with the middle of the plate. You must not take off 20 per cent. for the whole surface of the iron, because it was punched at the edges. He then gave the following table of results of testing four pieces of iron :

No. 1.

Piece of Lowmoor iron, 2 × $\frac{5}{16}$, cut from a plate longitudinally, or with the fibre ; distance between centre 3 in. ; commenced stretching at 10 tons.

At 11 tons stretched ·03
„ 12 „ „ ·08
„ 13 „ „ ·14
„ $13\frac{3}{4}$ tons broke ; size at fracture $1\frac{7}{8}$ × $\frac{1}{4}$ full
Limit of elasticity 16 tons per inch
Breaking strain 22 „

No. 2.

Piece of Lowmoor iron, 2 × $\frac{5}{16}$, cut from same plate as above, but transversly ; distance between centre $3\frac{1}{2}$ in. ; commenced stretching at 10 tons.

At 11 tons stretched ·04
„ 12 „ „ ·08
„ 13 „ „ ·16 and broke ; fracture 1 $\frac{15}{16}$ × $\frac{1}{4}$ full
Limit of elasticity 16 tons per inch
Breaking strain 20·8 „

No. 3.

Piece of G. B. Thorneycroft and Co's. treble refined iron, 2 × $\frac{5}{16}$, bar cut longitudinally from a plate ; $3\frac{1}{2}$ in. between centres ; commenced stretching at $9\frac{1}{2}$ tons.

At 10 tons stretched ·04
„ 11 „ „ ·09
„ 12 „ „ ·155
„ 13 „ „ ·25 and broke, size of fracture 1 $\frac{15}{16}$ × $\frac{1}{4}$
Limit of elasticity 15·2 tons per inch
Breaking strain 22·4 „

No. 4.

Piece of G. B. Thorneycroft's treble refined plate, 2 × $\frac{5}{16}$, cut transversely from the above; $3\frac{1}{2}$ in. between centres; commenced stretching at 11 tons.

At 12 tons stretched ·05
„ 13 „ „ ·10
„ 14 „ „ ·135
„ $14\frac{7}{8}$ tons broke; size at fracture $1\frac{13}{16}$ × $\frac{9}{32}$
Limit of elasticity 17·6 tons per inch
Breaking strain 23·8 „

He thought these results for soft ductile iron, was very satisfactory. He had tested some Swedish iron, which, stretched longitudinally, bore a strain of $32\frac{1}{4}$ tons to the inch, and transversely it gave a breaking strain of $27\frac{1}{2}$ tons. As regards the liability of flaws he had examined closely into that subject, and he found there was a series of flaws in each piece. He did not think he had ever seen a small plate (say) six inches long without one or two flaws. It was very difficult to find plates without a flaw.

Mr. Light inquired what means were employed for determining the permanent set.

Mr. Roberts said that the pieces were so short that he was obliged to use small dividers. During the last twelve months he had tested some hundreds of plates to be used in vessels for the Government, and he could assure the meeting that very few could stand 20 to 22 tons to the inch, others 21 to $21\frac{1}{2}$ tons, and sometimes it was difficult to obtain even that strength.

Mr. Le Feuvre thought the tendency of the present discussion was in favour of greater tests and a superior quality of iron being employed; but it was to be remembered that the Board of Trade test was higher than the tests required by several foreign governments.

For example, in France the test was 4 tons 10 cwt., in Italy 4 tons, in Holland $3\frac{3}{4}$ tons.

These tests were very low; but they were low, he thought, in consequence of the inferior quality of the iron in use.

As regarded bridges, he thought there was no question that the tests ought to be higher when superior qualities of iron were employed, and the several parts of a bridge had been actually submitted to a much higher strain.

The bridges recently built, or in course of construction, over the Thames, such as the Charing Cross Railway bridges, the Lambeth and the London, Chatham and Dover Railway bridge, were so designed that every portion of the bridge could be tested, with the exception of the cross girders, and under those circumstances there could be no question a much greater strain could be put upon the iron than upon arch girder or plate girder bridges.

Mr. Riley said it was obvious that flaws in plates would be more detrimental than in bars, as in bars the flaw was drawn out to an exceedingly fine thread, whilst in plates it was extended. In a case recently submitted to him respecting iron for tin plates, it was found that in the bad plates, which were much spotted all over with hard lumps, that very minute portions of charcoal were enclosed in the iron; this prevented the iron welding, and converted the iron contiguous to it into a hard steel, so that these lumps were transferred in doubling the plates, from one to the other; in the subsequent rolling of the plates this defect could never be remedied. Such defects as the above are only found where charcoal is used to refine the iron, and shows the great effect of any foreign matter in the iron when rolled into plates. The plates where these defects exist crack in bending, and make them almost useless.

In experiments on the tensile strength of iron, he thought that attention ought to be paid to the character of the ores used for manufacturing the iron, and also the process of manufacture employed, as it was only by these means that the smelter of the iron could obtain data upon which he would be able to improve the quality of the iron he produces.

Mr. Colburn said he had heard nothing to induce him to alter his opinion that there was great variation in the strength of iron; and there was reason to believe that the working strength of iron for working purposes was rather over estimated. He had recently been very much struck with a letter which had been published,

written by Messrs. Napier, of Glasgow, to Lord C. Paget, of the Admiralty, which letter spoke of 22 tons being a very high strain indeed. The experiments that were made were generally upon small specimens. He thought they were too much disposed to assume the strength of plates to be higher than it really was in practice.

The Government were testing all iron supplied at Chatham. A contract had been offered to the Staffordshire makers to manufacture plates upon the condition that they shall stand 24 tons to the inch, and it appeared impossible for the manufacturers to comply with the condition.

With regard to Bessemer's metal, so far as he (Mr. Colburn) knew, he did not think its elastic limit had been tested; indeed, Mr. Bessemer made so many different kinds of metal. He made the strong highly carbonized metal, and a lower quality for rails. The amount of carbon varied very much indeed, and this regulated the tensile strain and the purpose to which it was to be applied. Consequently, the exact proportion between the elastic limit and the breaking weight, could not be assigned; in fact, there was no definite proportion whatever, either three-fifths, two-fifths, or anything else. He believed all the facts proved this. Take the Swedish iron for instance; the elastic limit of that was very much lower than hammered iron. Take, also, certain kinds of Yorkshire iron; the elastic limit of which was 16 tons, and the breaking strain 20 tons; so that the elastic limit was very nearly four-fifths of the breaking strain. If they referred to Barlow's work "On the Strength of Materials," they would find that the elastic limit of one bar was put at 32 tons, and the breaking strain at 32½ tons.

The Chairman did not think it necessary to detain the meeting by any lengthened observations, although it was true that Mr. Colburn in his very able paper had not solved the difficult problem upon which it treated; yet, the question was so ably and clearly put before them that each could draw his own conclusion from it. From the extraordinary variation of iron in every possible way, it was difficult, if not impracticable, to arrive at any rule to guide them. There was no doubt that one of the first considerations in designing a bridge was to decide on the quality of the material with which it

was going to be built. He quite agreed with Mr. Colburn that iron varied in tensile strain from 15 tons to 30 tons to the inch, and that under such extraordinary range some strength must be assumed to which the parts would be tested. After the positive strength of the material to be used was decided on, they had next to consider the nature of the construction of that material. Messrs. Ordish and Carrington had touched upon that point. As regards the punching and drilling of plates, he (the Chairman) thought, before Mr. Roberts made his remarks, that the talented reader of the paper had left this point a little dubious. But it occurred to his (the Chairman's) mind, whether the extra cost of material necessarily required for punched work might not be less than the extra cost of labour in drilling and punched work preferable on this account; this view was quite overlooked by Mr. Ordish and others. He thought that Mr. Roberts's views were more correct with regard to punched plates, than those given by Mr. Light; that the strength could only be damaged by punching in proportion of the section punched, to the section left untouched in a girder, and that the loss of strength could not be the same for every construction of girder. Mr. Parsey had said that so long as the elasticity of the plate was not injured that it was safe. Mr. Parsey also said that he thought the elastic limit was put at rather too low an amount; but he (the Chairman) considered it was wise to do so. He understood Mr. Parsey to say that something like 11 tons, as the maximum strain, might safely be put on first-class iron; under these circumstances Mr. Parsey would be quite safe. He (the Chairman) thought that, as far as experiments were concerned, so long as the plate was undamaged, and the elastic properties were uninjured, there was no doubt they were perfectly safe; directly they got permanent set the iron was being damaged, and the strain was more than should have been applied. Another remark was made about the construction of bridges, all parts being designed so that every portion of the structure could be tested by itself before erection. Now that, no doubt, was thoroughly good in principle; for if they constructed a bridge of girders, and those girders were each carefully tested, they could be perfectly satisfied that the bridge would be safe, having not only the knowledge of the strength of the materials in the first place by calculation, but likewise an assurance of sta-

bility in the larger sections as separately tested. In conclusion, he might say that, although the present discussion had not enabled them to form any definite theory upon the subject, yet it had so ventilated it that the members of the Society generally would have a much clearer notion than hitherto of the important matter which Mr. Colburn had introduced in the elaborate paper read by him at their last meeting.

May 4th, 1863.

R. M. CHRISTIE IN THE CHAIR.

ON THE CONSTRUCTION OF CHELSEA BBIDGE.

BY GEORGE GORDON PAGE.

THE Chelsea Suspension Bridge, which has been opened to the public for the last five years, is a bridge remarkable in many respects, and which, in point of design, mode of construction, and economy of cost, presents features of great interest.

In the year 1846 an Act of Parliament was obtained, and the necessary funds granted for the construction of this bridge, which forms a communication between Pimlico, Belgravia, and Chelsea on one side of the river, and Battersea Park and the surrounding neighbourhood on the other.

In addition to the design for the suspension bridge the engineer, Mr. Page, was instructed to prepare, for the consideration of the Metropolitan Improvement Commission, designs both for a bridge of seven arches, faced with stone, and one in cast-iron of five arches; but, ultimately, the Chief Commissioner of Her Majesty's Works decided to carry into execution the suspension bridge originally mentioned in the Act.

In point of construction this bridge deserves particular attention; the whole of the details having been worked out with great care. Not only were the latest improvements made available, but many important innovations, calculated to ensure the stability of the structure, were introduced. The foundations, which will presently be

described, are similar in principle to those of Westminster new bridge, which were the subject of much discussion in Parliament a few years back. By the method adopted coffer-dams are entirely dispensed with.

The use of suspension bridges has of late increased, and is still much on the increase. Within the last few years three metropolitan bridges on this principle have been erected over the Thames, and application has been made to Parliament to erect more. The advantages of a suspension bridge are—the economy with which it can be constructed, and generally its non-interference with the bed of the river. It offers less impediment to navigation than any other description of bridge, and can be thrown across openings where it is impracticable from the altitude of the banks, the rapidity of the current, and other conditions, to erect centering for the construction of stone and other bridges. It can be erected with greater ease and expedition, and generally at much less cost than bridges constructed on other principles.

As regards the comparison of a suspension bridge and an arch, with a girder of the same span, deflection (or depth) and strength, it is evident that while the arch will correspond with the upper member of a girder, the chains of a suspension bridge will correspond with the lower member or tie of the girder; and, therefore, a great advantage is possessed by the suspension bridge and arch bridge, in point of weight, over the girder—a girder being, as it were, a compound of the two constructions. The arch is a structure which requires so many additions to maintain its shape, that it cannot practically be looked upon as only being in a state of compression. The case of the suspension bridge is, however, quite different: the chains being in a state of equilibrium, preserve their form independently of any additional assistance; the only disadvantage of the suspension principle, as applied to railway purposes, is its flexibility: but even this has been successfully overcome in the case of the Niagara bridge, built under the direction of Mr. Roebling.

This bridge crosses the Niagara river at a height of 245 ft. above the level of the water by a single span of 821 ft.; its rigidity is owing to the deep trussing, 18 ft. deep between the upper and lower ways.

It should be borne in mind, that suspension bridges are incomparably slighter than bridges of stone and cast-iron. A bridge intended to be a great and perpetual thoroughfare, exposed to crowds and heavy goods' traffic—in short, a bridge in the position of London Bridge, should not be on the suspension principle. If the strength were increased to that of an arched bridge, such as London or Southwark, the extra weight, the difficulty of erecting the chains, and the increase of the foundations, would so raise the expense that it is doubtful whether it would not be more economical to build an arched bridge.

For large openings, where it is important to have a permanent passage, and where the traffic is not very great, as at Lambeth, suspension bridges are admirably fitted, as they can be constructed of almost any span, and at any height, with greater economy than bridges on any other principle.

As regards preservation, every link, every bolt, every bearing, from the moorings on one side to those on the other, are, or should be, in a properly constructed suspension bridge, open to inspection, and can at any time be examined, and preserved with paint or other material.

GENERAL DIMENSIONS.

The length of Chelsea Bridge is 704 ft. from face to face of abutments; and consists of a centre opening of 333 ft., with two side openings 166 ft. 6 in. each. The piers are 88 ft. long, and 19 ft. wide, terminating in curved cutwaters; the piers are carried to a height of 7 ft. 6 in. above high-water mark, the width of the bridge is 47 ft., the roadway at the centre of the bridge is 24 ft. 6 in. above high-water, and has a curve of 18 in. rise, commencing at the abutments. The towers and ornamental casings are of cast-iron. The girders and flooring of the platform of wrought-iron.

OF THE ABUTMENTS.

Too much attention cannot be bestowed on the abutments of a suspension bridge, as on their careful consideration and construction so much depends.

The abutment is the mass of masonry, or in some cases of natural

rock, to which the extreme ends of the chains are made fast, and by the weight of which the strain from the chains is resisted.

The principles of the stability of the abutment of a suspension bridge, are the same as those of the abutment of an arched bridge, but reversed.

In the former there is a tendency to upset or slide forward instead of backwards, as is the case in the latter. The weight or gravity of the abutment should always be sufficient to prevent it from sliding on its base, and its form and dimensions should be sufficient to prevent it from upsetting. The tendency to sliding forward may be considerably lessened by making the base of the abutment, or a portion of it, slope so as to be at right angles, or nearly so, to the resultant of pressure.

Of all parts of a suspension bridge, the abutments are the last in which solidity and stability should be sacrificed to motives of economy.

The weight of the abutment should be equal to resisting twice the utmost strain that can be brought upon the chains by dead weight, and the total power of resistance, combining the weight and the tendency of the abutment to slide from the ground on which it stands, should be at least equal to four times the utmost strain that can be brought upon it.

The resistance offered by the adhesion of the abutment to the ground on which it stands depends entirely upon the nature of that ground, and cannot by any general rule be accurately predetermined.

When piles are used in the foundation, they should be driven at an angle approaching as near as possible to the direction of the resultant of pressure.

With regard to the saddles on the abutment, by the aid of which the direction of the chains is changed, it is not always necessary to place rollers under them, but as they must be capable of sliding to a sufficient extent, other means are sometimes resorted to in bridges of short span, and the saddles are sometimes laid on a bed of asphalted felt. In large suspension bridges rollers are, however, universally used, to allow for the expansion and contraction of a necessarily large extent of chain.

As it is most important that the chains or wire cables of a suspension bridge should be kept free from rust, the tunnels in the abutment through which the chains pass down to their fastenings are generally constructed of such dimensions as will allow of space for access for the purposes of examination and repair if required.

The abutments of the Chelsea Bridge consist of a mass of brickwork and concrete, measuring at the base 112 ft. in length, by 56 ft. broad, and at the top 100 ft. by 46 ft. and 40 ft. deep.

The face of the abutment adjoining the river is composed of cast-iron piles and plates, somewhat similar to those of the pier, with the exception that the ironwork is not brought above the level of low water.

The portion of the abutment on which the land saddles and cradles bear, for changing the direction of the chains, rests upon timber piles, 14 in. square, driven deep into the bed of the river, and are from 3 ft. 2 in. to 4 ft. from centre to centre; these piles are cut off at the level of low water, 16 ft. below Trinity high-water mark, and the spaces between filled up with hydraulic concrete; the cast-iron and timber piles are tied together with wrought-iron ties 3 in. × $\frac{3}{4}$ in. On the top is bedded a series of landings, forming a table at the level of low water 53 ft. 6 in. × 27 ft. × 6 in., upon which a mass of brickwork is erected, up to a mean level of 3 ft. below the level of the roadway—upon this, 12 in. landings are bedded for the reception of the cradles which carry the saddles on rollers; the cradles are bedded in asphalted felt, and firmly secured by wrought-iron holding-down bolts, brought up through the masonry from below. An invert, springing from beneath each saddle, is built in the brickwork below, so as to distribute equally the pressure from the cradles over the whole area of the foundation.

The mooring chains are carried down tunnels to the moorings, the tunnels forming an angle of 155 deg. with a horizontal line. The chains are secured to massive cast-iron mooring plates, resting against three courses of 12. in. landings, respectively 12 ft. × 8 ft. 9 in., 16 ft. × 12 ft. 6 in., and 20 ft. × 16 ft. 3 in. The tunnels are contracted at the bottom by elliptical brick domes, thus affording a complete bearing for that portion of the landings at the end of the tunnel. These landings rest against a mass of brickwork, with inverts, to distribute the pressure over the whole area of the abutment.

This mass of brickwork rests on a series of timber piles, driven at an angle of 65 deg., with a horizontal line; the tops of the piles coming up above the level of the concrete, and having a good bond with the brickwork, by which means the tendency to slide is greatly diminished, the whole spaces between the masses of brickwork being filled up solid with concrete.

The Pier Foundations.

The construction of the foundations of the piers combines all the advantages of foundations on bearing piles, made by means of cofferdams, without the expense and obstruction to the waterway which they involve, and which would have rendered their use at Westminster Bridge all but impracticable.

The foundations of the piers consist of timber bearing piles 14 in. square, driven deep into the bed of the river at intervals of 3 ft. over the whole area of the pier, varying in depth from 40 ft. 6 in. to 25 ft. below the level of low-water, according to the resistance offered by the bed of the river.

The face or external surface of the piers consists of a cast iron casing of piles and plates driven alternately; the main piles are 12 in. in diameter and 27 ft. long, with longitudinal grooves on each side for the reception of the plates. These piles are driven to a uniform depth of 25 ft. below the level of low-water, and between them are driven cast iron plates or sheeting 7 ft. 2 in. wide, so that the pier is entirely cased from the foundations to the top, which is 7 ft. 6 in. above Trinity datum. The space enclosed by this casing is then dredged to the hard gravel above the clay, and filled in solid with concrete up to the level of the top of the timber piles. On this foundation a flooring of stone landings is bedded, and on this the cast iron plates, frames, &c., forming the base of the towers are placed.

The portion of the caisson situated above low-water is hollow, being so formed to avoid throwing useless weight on the foundation, and is merely lined with brickwork, strengthened by cross walls and iron ties.

The whole of the ironwork below the water was covered when hot with a protecting coating of tar. The thickness of metal in the caisson is 1 in.

Of the Towers.

The towers which support the chains are entirely independent of the ornamental cast iron casing surrounding them, and consist of a cast iron columnar framing strongly braced both horizontally and vertically, carried to a height of 57 ft. above high-water.

The columns are cast in pairs and have a diameter of 10 in., and thickness of metal of 1 in. They are arranged in clusters of fours, and the whole are connected with six horizontal frames, occurring at intervals. The columns are not vertical, but incline towards each other upwards from either side of the pier, the columnar framing being 13 ft. 6 in. at the base, and 9 ft. 9 in. at the top. In the direction of the piers the columns are 4 ft. 3 in. apart, and rise parallel to each other. There are two towers on each pier, 32 ft. from centre to centre. The pressure from the chains coming directly from their centre, each tower carries therefore one-fourth of the whole weight of the bridge, or about 375 tons, or about 670 tons when the bridge is completely loaded; the sectional area of the columns is 284 square inches, and there is, therefore, a pressure upon them when the bridge is loaded of 2·36 tons per square inch of section. The weight of the towers, exclusive of the ornamental cast iron casing, is 350 tons.

On the towers are fixed massive cast iron cradles upon which the saddles rest.

Of the Platform and Roadway.

The roadway platform is carried by two longitudinal trellis girders, running the whole length of the bridge from abutment to abutment, immediately beneath the chains, by which they are supported at intervals of 8 ft. These girders are suspended from the chains by wrought iron rods 2 in. in diameter.

The weight of the roadway is distributed over the whole of the four chains by the coupling plates to which the rods are attached; the rods are jointed at the chains and at the roadway to accommodate any lateral motion that may occur, and are provided with screw coupling boxes for their adjustment; the suspension rods pass through the longitudinal trellis girders, and support them from beneath.

The transverse girders which support the roadway are placed 8 ft. apart from centre to centre, immediately under the suspension rods, and bear upon the bottom flange of the longitudinal girder, are 31 ft. 10 in. long, 2 ft. $2\frac{3}{4}$ in. deep at the centre, and 1 ft. 11 in. at the ends, where they are connected by a system of rivetting with cantilevers 7 ft. long, which practically form a continuation of them, and serve to support the overhanging footpaths; the sectional area of the top and bottom flanges is 10 in., and the vertical rib $\frac{1}{4}$ in. thick stiffened with T-iron

The small roadway bearers between the transverse girders are from 3 ft. 3 in. to 3 ft. 10 in. apart, 8 ft. long, and vary in depth from 1 ft. $5\frac{3}{4}$ in. to 1 ft. $9\frac{1}{2}$ in. to suit the cambered surface of the roadway.

The several girders that support the roadway thus form a series of rectangular cells, which are covered with arched plates of wrought iron, stiffened with angle iron.

The haunches of the plates are filled in with a light concrete, composed of cork and bitumen.

Previous to laying the bitumen concrete, the plates and girders are coated with asphalte.

The roadway is paved with oak blocks 6 in. × 3 in. × 4 in., bedded in bitumen, and trams of timber, flush with the roadway, with wrought-iron strips bolted down on the top for durability.

The preference was given to the cork and bitumen concrete as a bedding for the roadway blocks, on account of lightness compared with ordinary concrete. Concrete, moreover, in such a position, and in so thin a layer, is liable to crack, and become in time pulverised, and (then no better than loose gravel) liable to be deranged by passing traffic.

The footpaths are paved in the same way, only the blocks are of smaller dimensions. This pavement rests on planking placed on joists running longitudinally, resting on the cantilevers. The available breadth of the carriage-way is 29 ft., and footpaths 14 ft. 4 in.

The longitudinal trellis girder is 6 ft. deep, and its flanges are composed of a top plate 10 in. + $1\frac{1}{4}$ in., and two angle irons $3\frac{1}{2}$ in. × $3\frac{1}{2}$ in. × $\frac{5}{8}$ in. thick, the effective area of the top and bottom flanges is $12\frac{1}{4}$ square inches.

This girder materially stiffens the roadway and prevents, in a great degree, that undulation to which suspension bridges are liable.

The handrail is of wrought iron, secured to the cantilevers at every 8 ft. by brackets. The ornamental bosses and stays for supporting the railing are of cast iron.

The Chains and Saddles.

The chains of the Chelsea Bridge are four in number, two being placed on either side, at a distance apart of 32 ft. They consist of links of seven and eight bars alternately, 8 in. wide, and of lengths varying from 16·55 ft. at the towers to 16 ft. at the centre of the span, so as to admit of an uniform horizontal distance of 16 ft. from centre to centre of the pin-holes of each link, and are connected by pins $4\frac{1}{4}$ in. in diameter. The aggregate section of the four chains at the towers is 230 square inches, and at the centre $217\frac{1}{2}$ square inches. The span of the centre opening is 348 ft., and the deflection of the chain is 29 ft. The semi-span of the back chains is 183 ft., and the deflection 30 ft. 6 in. The length of the chain for the centre opening is 354 ft. 5 in., and the length of each of the back chains 186 ft. The mooring chains are placed at an angle of 25 deg., and are 95 ft. long, and have an aggregate section of 235 square inches. The total weight of the chains is 340 tons. The chains are carried over the towers by means of saddles formed of No. 8 1 in. wrought iron rectangular plates, 5 ft. 8 in. long, and 2 ft. 10 in. wide, placed at intervals of 1 in. apart, and bolted together by No. 10 bolts. The bottom edges of the plates are planed, and are let into a cast-iron plate 4 in. thick, also planed on its top and bottom surface, and which moves on ten 6 in. diameter steeled rollers, working on the cast-iron bed-plate fixed at the top of the towers. The chains are connected to the saddles in the same way as the links of the chains are connected together. At the abutments the chains are diverted down the tunnels by means of saddles of similar construction to those on the towers, based on cast-iron cradles, and placed at right angles to the resultant of the strains.

For mooring the chains the following means were adopted:—As has been observed in the description of the abutments, the tunnels for the mooring chains are closed at the bottom by elliptical-shaped

brick domes, against which the York landings are placed at right angles to the angle of inclination of the mooring chains. The chains pass through holes formed in the centre of the landings (the dimensions of the landings were stated in the description of the abutments). A brick semi-circular arch or invert springs from the outer face of the landings, and connects the two sets of landings of each abutment together, by which means the whole weight of the middle portion of the abutment, it will be seen, is made to resist the pull of the chains. The chains are secured by means of castings 21 in. deep, abutting against the landings, and are divided, each into four compartments, rather more than two inches wide, through which the chain bars (here put two and two together) pass, and are moored by keys driven through the heads of the bars, and bearing against the mooring castings. Keys were here used instead of pins to allow of an adjustment in the length of the chains. Similar means for adjusting the lengths of the chains were made at the saddles on the towers, but were not needed.

In calculating the length for the chains the curve may be assumed to represent a parabola, though, strictly speaking, the curve of the chains is peculiar to the construction; but deduction being made for the stretch due to the tension, caused by the appended weight, the weight so deducted will be found practically correct. Care should be taken to ascertain the exact distance of the span, as a small error in the horizontal distance will cause a serious error in the amount of deflection. It is well to provide for any discrepancy of this kind by leaving the centre links of the chains the last to be rolled; when the error being known it can be rectified without any serious interference with the rest of the construction.

For the erection of the chains four temporary chains were thrown across made of 2 in. round bar iron, and placed one on each side of the line of the chain to be erected. Upon these temporary chains travelling purchases worked; by which the bridge chains were hoisted and put in place. Four other and similar chains were thrown across beneath the former mentioned ones to which timber platforms were suspended, and which served to carry the bars of the chains until the connection of the links was complete. In the hope that the descrip-

tion may be acceptable, a few observations are subjoined respecting the manufacture of the bars.

The bars for the chains of the Chelsea Bridge were manufactured by the process patented by Messrs. Howard and Ravenhill, by which the head and body of the bars are rolled of one piece, and was effected as follows:—Piles or, as they are technically called, balls of cleansed scrap iron, of about ¾ cwt. each, were heated (eighteen balls being the usual charge) in a reverberatory furnace of ordinary construction, and afterwards hammered into slabs about 2 in. thick by a 4-ton wrought iron hammer. The slabs, while still hot, were then piled in sets, of the weight required for the respective bars, and again heated and hammered into oblong masses of iron called shingle, somewhat wider than the width for the bars, and about 2 ft. 9 in. long. The time required for heating the balls of scrap was one hour and a quarter, that is, so much time elapsed from the time of charging the furnace to the withdrawal of the first ball; and the time required for hammering the eighteen balls into slabs was three-quarters of an hour. It may, therefore, be observed that the last ball withdrawn was nearly twice as long in the furnace as the first ball was; and it may, consequently, be supposed that some of the balls of scrap were too much and others too little heated, but the precautions adopted in the management of the furnace prevent any great irregularity in this respect. The balls first withdrawn were placed nearest the furnace, and, as withdrawn, the remaining balls were pushed nearer the furnace, or otherwise, as their state required. The time for hammering a pile of slabs into shingle was about five minutes. By the two heats and hammerings the loss of iron was about 13 per cent.; and after the shingle was rolled into bars the total loss of iron was 20 per cent., that is, the bar weighed one-fifth less than the scrap iron weighed from which it was manufactured. For converting the shingle into bars of the required form the shingle was heated to the required temperature in the furnace of the rolling mills, and was then passed longitudinally through rollers till reduced to a width of 8 in., and to a thickness of 2¾ in. It was then transferred to other rollers, and passed through sideways—these rollers being so constructed as to act only on the extremities of the bar, which, by this means, were

spread out to the width required for the heads. The bars were then passed again longitudinally through ordinary rollers, till reduced to the length and thickness required; after which, while still hot, they were straightened by being beaten with wooden mallets. The time required for rolling a shingle into a bar was eight minutes.

The next process was boring the pin-holes. In doing this the bars composing each link were placed one on another, and bored by one operation, by which means uniformity of length was obtained. Shearing the heads of the bars to the proper form was the next operation. To do this the bars were fixed eccentrically on a table revolving in contact with shears, which, as the table turned, cut off the superfluous portions of the heads.

Every bar of the chains at this stage was tested with a strain of $13\frac{1}{2}$ tons per square inch, the contract requiring, in order to insure material of the best quality, that the iron used should stand this strain without a permanent elongation of more than one-fortieth of an inch in a ten-feet length. It having been found from experiments made that up to this strain the best commercial iron did not extend more than the very best iron that could be manufactured. It may be observed that notwithstanding this amount of strain very few of the bars had to be rejected.

The last process in the manufacture of the chains was numbering the bars and lettering the links, that there should be no mistake in erecting the chains, as to every bar being in its proper place. A few words will suffice to explain how this was carried out. The chains were divided into eight portions and named A, B, C, D, E, F, G, H, respectively. The chain A extended from the moorings on one side to the centre of the bridge, where it was joined by the chain B, which continued to the moorings on the other side, and so of the other three remaining chains. The heads of every bar of every link were then stamped with the letter of the chain to which it belonged and numbered—the heads of the first links at the moorings being numbered 0, and the heads at the other extremity of these links 1. The heads of the second series of links were numbered 1 and 2; of the third series 2 and 3, and so on throughout the whole length of the chains. The bars of every link were also numbered 1, 2, 3, 4,

5, 6, 7, 8, showing the position they occupied in the link during the operation of boring.

The engineer considered it highly advantageous to the successful completion of this part of the bridge that the chains were prepared by Messrs. Howard, Ravenhill, and Co., who spared no pains and no expense to carry out his instructions to produce a perfect structure; and so far from their making any attempt to evade any condition of the contract for their own advantage, the perfection of the work was their chief consideration.

It will show the excellence of the iron they produced to state that whereas the late Mr. Barlow deduced that the stretch of iron was at the rate of one-ten-thousandth part of its length for each ton, the iron which Messrs. Howard, Ravenhill, and Co. produced for the chains of the bridge only stretched from one-fifteenth-thousandth to one-fourteen-thousandth part of the length per ton, being above 50 per cent. less than Mr. Barlow's.

As so much depends upon an honourable contractor in the execution of a work, Mr. Page authorised me to make these observations in justice to Messrs. Howard, Ravenhill, and Co.

Of the Probable Load.

Before considering the degree of strain to which the chains are liable, it would be well to investigate the amount of load to which a bridge may be subjected.

M. Navier, a great authority on suspension bridges, calculated the load likely to occur on a bridge at 42 lb. per square foot. The standard proof for suspension bridges in France is 200 kilogrammes per square mètre, which amounts to 41 lb. per square foot, the proof load required by the French Government.

For troops on march 21 in. in rank and 30 in. in pace are allowed, giving 4·37 superficial feet per man, which, at 11 stone each, would be $35\frac{1}{4}$ lb. per square foot.

The load taken in the calculations for the Menai Bridge was 43 lb. per foot super.

An experiment was made by the engineer of the Chelsea Bridge by packing picked men on a weigh bridge, with a result of 84 lb. per

superficial foot, but it is not within the limits of probability that such a crowd could accumulate on any bridge.

Seventy pounds per square foot of platform are assumed as a standard for the load that may come on a bridge, as being the utmost load that the platform could hold, supposing it, in fact, quite filled with people crowded as close together as they could be. This, it is true, is not often likely to happen, but it may do so on a public occasion, and needs, therefore, to be provided for.

The march of cavalry, or the passage of cattle, is not so productive of dangerous effects as troops on the march, inasmuch as cavalry take up more room in proportion to their weight, and do not preserve a uniform pace.

As regards the greatest moving load or crowd, it is an acknowledged fact that it is impossible for a body of people on the move to occupy per man less space than trained troops, and as I have before shown that troops on the march do not produce a greater dead weight than $35\frac{1}{4}$ lb., one may safely assume that the dead weight due to a moving crowd will not amount to so much.

Of the Strain on the Chains.

Having described the various loads that may come upon a bridge, it may be useful to show the strain produced on the chains of the Chelsea Bridge under the several circumstances.

The strain on the chains from their weight alone is 1·1 tons. The strain from the weight of the platform and road alone is 3·32 tons, giving a total strain produced by the structure alone of 4·42 tons, or 9·08 tons below the proof strain.

The strain on the chains from the weight of the structure and a load of 70 lb. (being the weight per square foot of a dense crowd), is 7·60 tons—or 5·9, nearly 6 tons, below the proof strain; so that the chains will carry in addition to the weight of the structure nearly three times the greatest crowd that can come upon the bridge, before the proof strain is arrived at. Taking the breaking strain of the chains at 28 tons, we should require $7\frac{1}{2}$ times the greatest possible load to be brought on the bridge to produce that strain.

Before concluding these observations on the Chelsea Bridge it may be interesting, without taking into consideration the high quality of

the iron, to compare the strain on the chains with other suspension bridges; and for this purpose I may refer to the Hammersmith and Pesth Bridges as fine examples of bridge engineering, both being built by the same engineer, Mr. Tierney Clarke, at distant intervals —the Hammersmith Bridge having been open thirty-six years, and the Pesth fourteen years.

The Hammersmith Bridge is 710 ft. 8 in. between abutments, the span of the main opening is 442 ft. 6 in., the deflection is 29 ft. 6 in., the useful width of platform is 30 ft., the sectional area of the chains is 180 square inches, the weight of a square foot of road 63 lb., and the strain per sectional inch upon the chains from a load of 70 lb. is 8·86 tons; the chains were proved up to 9 tons, leaving a margin of ·14 tons between the proof strain and the strain from the greatest load.

The Pesth Bridge is 1262 ft. between abutments, the central span is 666 ft., the deflection of the chains is 47 ft. 6 in., or $\frac{1}{14}$th of the span, the available width of roadway is 36 ft. 3 in., the weight of a square foot of suspended roadway is 74 lb., and the chains have a sectional area of 510 square inches.

The strain produced on the chains with a load of 70 lb. per square foot is 7·72 tons, or 1·28 tons below the proof strain, all the bars having been proved up to 9 tons.

The margin or allowance between the strain from the greatest load and the proof strain is therefore as follows:

Hammersmith Bridge	·14 tons.
Pesth Bridge	1·28 tons.
Chelsea Bridge	5·9 tons.

DISCUSSION.

Mr. CARRINGTON said he was very much pleased with the plan that had been adopted for the abutments, and with the arrangement of the piles. Driving the piles on the slant, he believed, was advantageous. As regarded the load on a bridge, he considered that 70 to 80 lbs. per square foot was certainly as much as could fairly be put upon the roadway. It could scarcely be imagined that there ever would be a crowd over the whole of the bridge so great as to produce a load of much more than 70 lbs. to the square foot. He thought 70 lbs. was a very fair allowance, but there was no harm in taking 80 lbs. to the square foot, that load would certainly give a greater margin for safety. He understood the author of the paper to say that, in France 200 kilogrammes per square metre was taken as the working load, and 400 kilogrammes as the test load; those loads were taken by both French and Italian engineers, the load of 400 kilogrammes was simply a test load to be put upon the bridge for a short time, in order to make it quite certain that the bridge was strong enough for the continuous working load of 200 kilogrammes per square metre, equal 41 lbs. per square foot. Referring to Chelsea Bridge, he should like to ask how it was that the road plates had corroded so rapidly?

Mr. C. J. LIGHT asked for information as to the connection of the suspension rods with the chain.

Mr. F. C. REYNOLDS said that in all structures where links of an uniform thickness were used, connected together by round pins, attention was requisite to make sure that there should be a sufficient bearing surface at the back of the pin, and if, as was frequently the case, the pin had a sectional area equal to the section of the bar, it would very often happen that the bearing surface before referred to would be far from sufficient; for instance, assume a bar 10 inches by 1 inch = 10 in. area with a round pin of the same area—*i. e.*, about $3\frac{5}{8}$ in. diameter—then, even on the assumption that the half circumference would be effective, the bearing surface would only be about $5\frac{7}{16} \times 1 = 5\frac{7}{16}$th area to sustain the pressure that could be put on by a bar of 10 in. × 1 in. = 10 in. area; the bearing surface was, therefore, evidently inadequate. The remedy was obvious enough,

viz., either to increase the diameter of the pin beyond what was required for the strength of the pin itself, or else to thicken the head of the bar so as to give more surface for the same diameter of pin. He believed this was a matter of the greatest importance, but one that was very frequently overlooked; it came prominently forward in the case of suspension bridges, but had often to be considered in designing warren and other trussed girders.

Mr. Zerah Colburn had come quite unprepared to take any part in the discussion of the paper just read. The paper was of a very interesting character, and very complete in point of description. There was one point in connexion with Chelsea Bridge which the author had not, he (Mr. Colburn) thought, sufficiently adverted to, namely, the ornamental character of that bridge. That certainly was a point which should not be forgotten, although it had not been mentioned by Mr. Page; and the more especially, when so many bridges were being constructed, which certainly did not possess any merit of that description.

Mr. Parsey said that, at the Institution of Civil Engineers the previous week, the question of strains and the measure of the load was introduced. Mr. Cowper stated that he had placed a number of men together, and they weighed 140 lbs. to the square foot, and from that time he had adopted that measurement. Another gentleman said that he always took 100 lbs., and another that he always took 80 lbs. Admitting every possible variety in the weight of men he (Mr. Parsey) thought that, upon an average, when they were put together closely, that they would weigh, at least, 112 lbs. per square foot. But in ordinary crowds of people, he thought, 80 lbs. might be taken as being sufficient. Upon the continent it was not usual to take quite so great a load. As regards the strain per square inch on the parts of a bridge, 7½ tons was the greatest that could be applied. The point which had been alluded to by Mr. Reynolds, namely, the size of the connecting pins, was one worthy of a considerable amount of attention. It had always appeared to him (Mr. Parsey) that the view generally taken of the case was erroneous. The pressure acted on a surface equal to the diameter of the pin multiplied by the thickness of the bar, so that if the pin was two-thirds of the width of the bar in diameter, and a strain of five tons

per square inch on the body of the bar, the pressure at the back of the pin hole would be $7\frac{1}{2}$ tons. He had seen instances where the back of the pin hole had been upset the 50th part of an inch, although the other parts of the bar were uninjured. He would ask Mr. G. Page whether he had tested any of the links, and if so, whether he found them equal to 28 tons to the inch.

Mr. F. Young, referring to the difference in the weight of men, said that he reckoned fifteen average sized people to the ton as a fair average. He did not agree with Mr. Cowper, for he (Mr. Young) thought 80 lbs. to the square foot was quite outside the mark, and safe in practice.

Mr. G. Page, said that a question had been asked concerning the roadway plates, and allusion had been made to the rusty condition in which they were. All he (Mr. Page) could say in reply was, that they had not been painted for a considerable time; indeed, the bridge had not been painted for five years. With respect to the question of the connection of the suspension roads with a chains, this was effected in a somewhat similar way to the plan adopted at Hungerford-bridge. The suspension rod was divided into two at the chains by means of a cross head, or short bar attached at the centre to the suspension rod, and at the ends to the two short rods, one being attached to the upper chain and the other to the lower, by which means the leverage is the same on each side, and consequently the strain is the same. As regards the size of the pins, they were of the general proportion of the pins used in suspension bridges. The diameter of the pin was $4\frac{1}{4}$ and the depth of the bar 8 inches. Very great attention was paid to this point, and it was not until after several experiments had been made that the size of the pin was determined upon. As regards not having made any observations with reference to the ornamental parts of the bridge, he did not consider that was a question of engineering. He had confined his paper strictly to engineering points, and left people to judge for themselves as to the ornamental portion of the bridge. As to weight, it was considered by Mr. Page that the greatest load that could be produced would be 70 lb. to the inch. They had tested bars made in a similar manner, and 27 or 28 tons was a very good average of the

breaking weight. It was not till after several experiments were made to prove the load that it was determined upon.

The Chairman said he was afraid the very quality of Mr. Page's paper was one great reason why there had not been such a lengthened discussion. He was sure every member would heartily thank Mr. Page for the truly practical paper he had submitted. It was a paper which, when published in the Society's Transactions, would be even more appreciated than it was now, for the members had not yet had an opportunity of fully considering these practical details. For the first time in the history of this Society, the committee had determined to publish the whole of the Transactions, and the first book would be ready for issue in the course of a few weeks. As regards Mr. Page's paper, he had already expressed his opinion as to its merits; and he (the Chairman) thought with Mr. Page that 70 to 80 lbs. must be taken as a fair load, for Mr. Cowper's 140 lbs. could only be an exceptional case. Mr. Page in his paper had entirely omitted the very important part of the structure, its ornamentation. That certainly might be called an engineering portion of the structure; the more especially was this so in connexion with Chelsea Bridge. Its ornamentation was quite an innovation, something new in Bridge Architecture about London, and he thought they should be all obliged to Mr. Page for having taken such a decided stride. Mr. Colburn had referred to the want of this important point in the bridges at present being constructed about London, and he (the Chairman) thought it was a pity that such ugly structures were allowed. It was singular that the legislature, or those Local Boards who had authority, had not taken this matter in hand, so that these bridges could have been made pleasing to the eye, and at the same time meet the great exigencies of the public traffic.

CHELSEA BRIDGE.

REFERENCE TO PLATES.

PLATE I.—Half Elevation of Bridge.

„ II.—Longitudinal Section of Abutment on the line of Chains.

„ III.—Part Plan, showing top of Abutment, Part Section along line of Chains, and Half-Plans of foundations of Abutment.

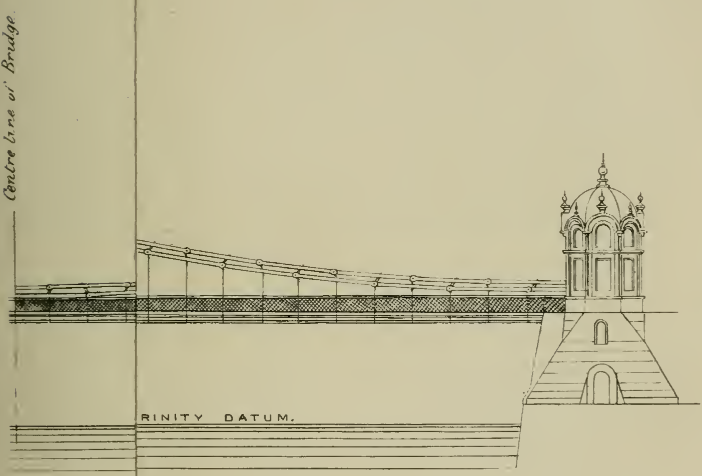
„ IV.—Plans, Elevation, and Sections of Cast Iron Piles and Plates used in the Pier foundations.

„ V.—Elevation of Towers.

„ VI.—Plans and Elevation of Horizontal Frames in Towers.

„ VII.—Half-transverse Section and Part Plan of the Platform and Roadway.

Plate 1.



G.G Page

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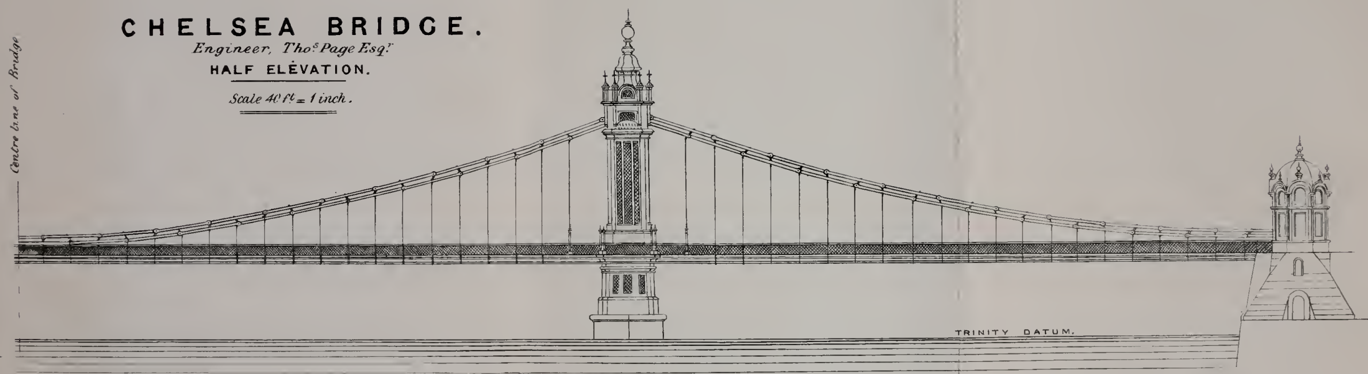

G.G. Page del.

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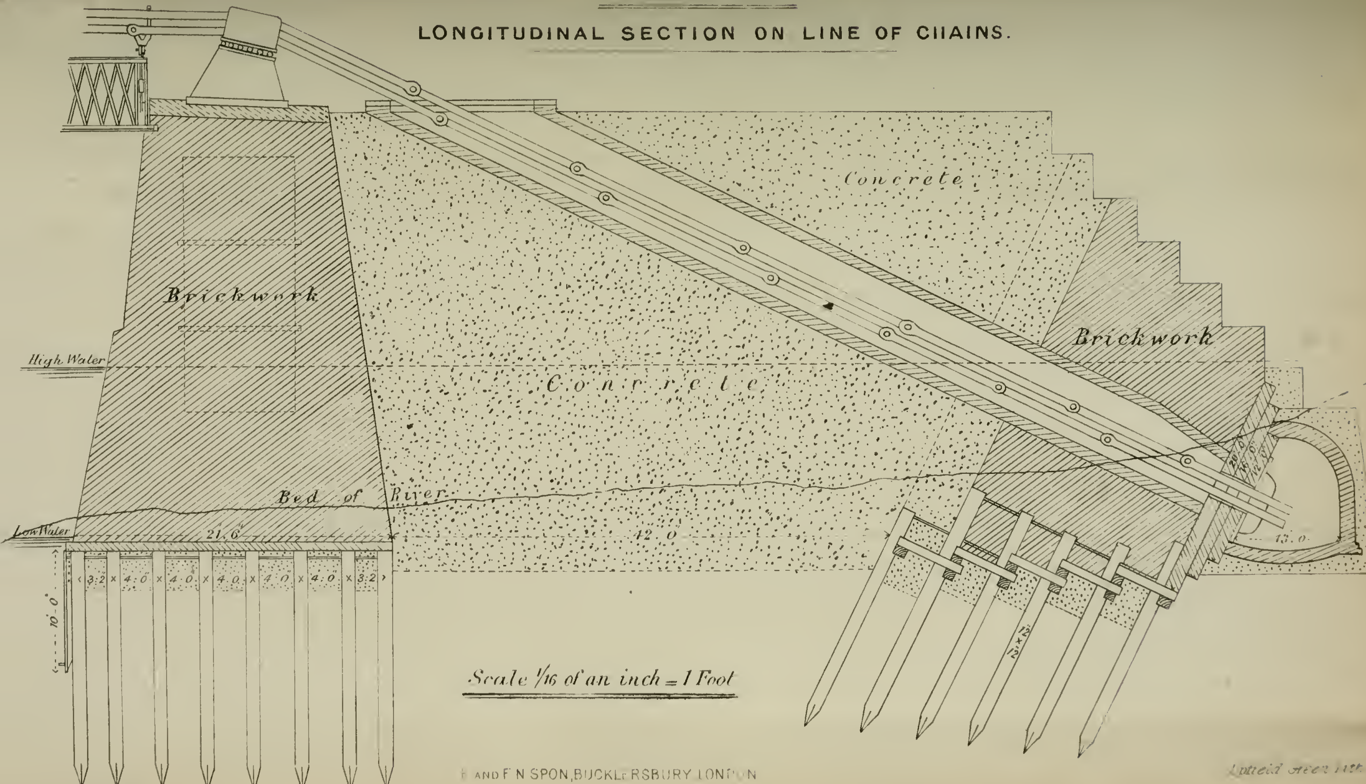
ABUTMENTS.

LONGITUDINAL SECTION ON LINE OF CHAINS.



Scale 1/16 of an inch = 1 Foot

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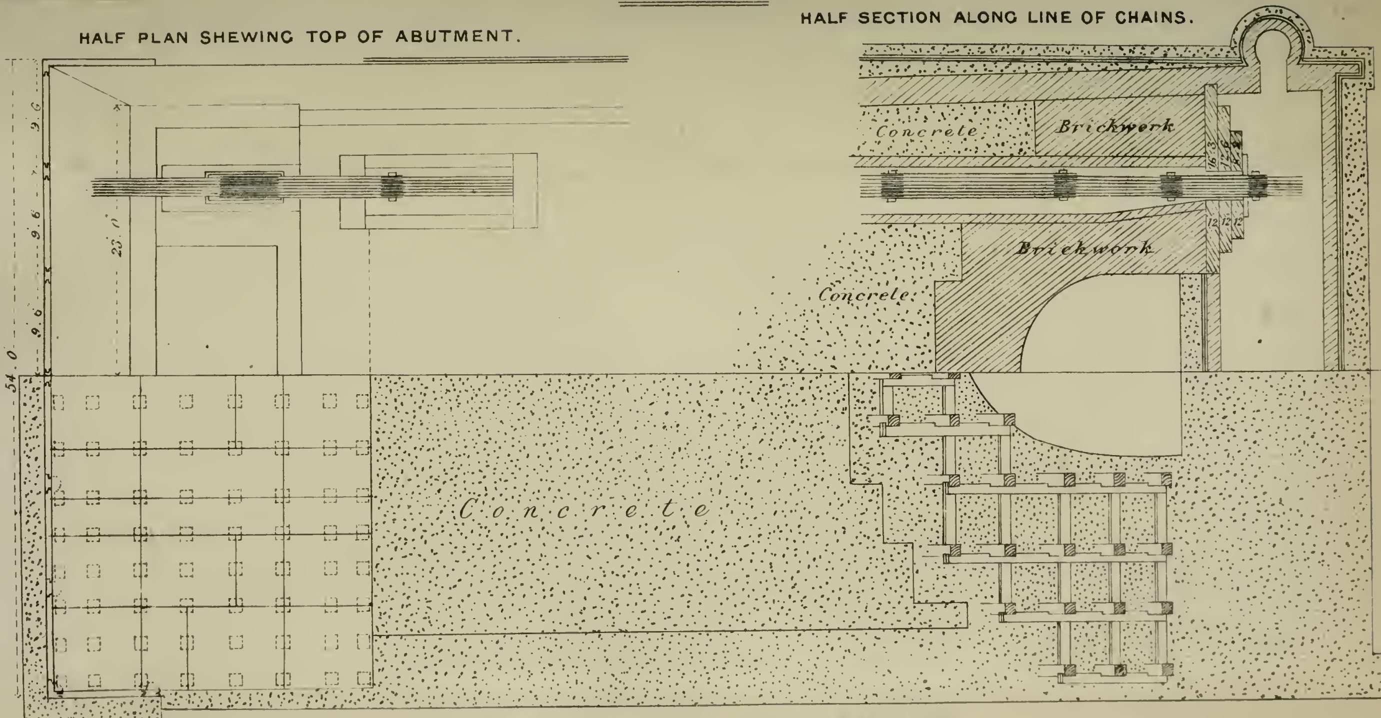
CHELSEA BRIDGE.

ABUTMENTS.

HALF PLAN SHEWING TOP OF ABUTMENT.

HALF SECTION ALONG LINE OF CHAINS.



HALF PLAN OF FOUNDATIONS.

Scale 1/16 of an inch = 1 Foot.

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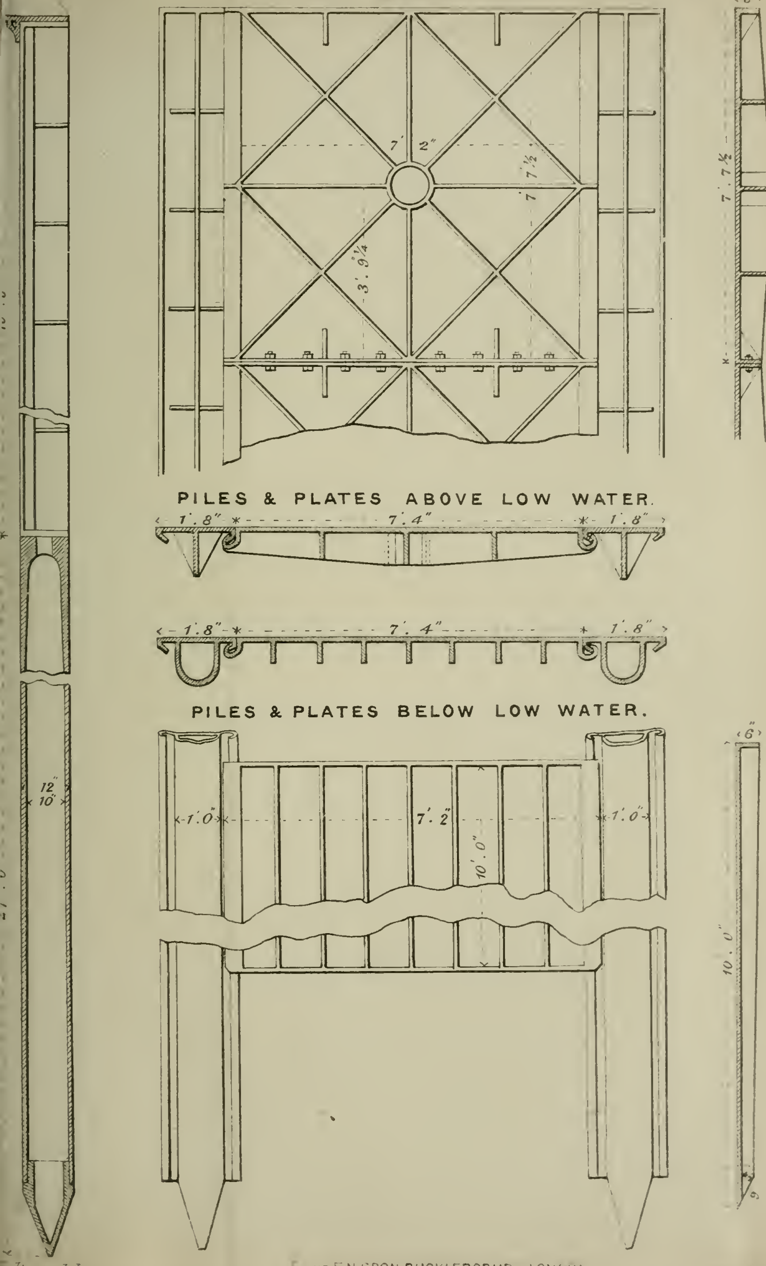
Plate 4

CHELSEA BRIDGE.

DETAILS OF PILES & PLATES IN FOUNDATIONS.

SCALE ¼ INCH = 1 FOOT.



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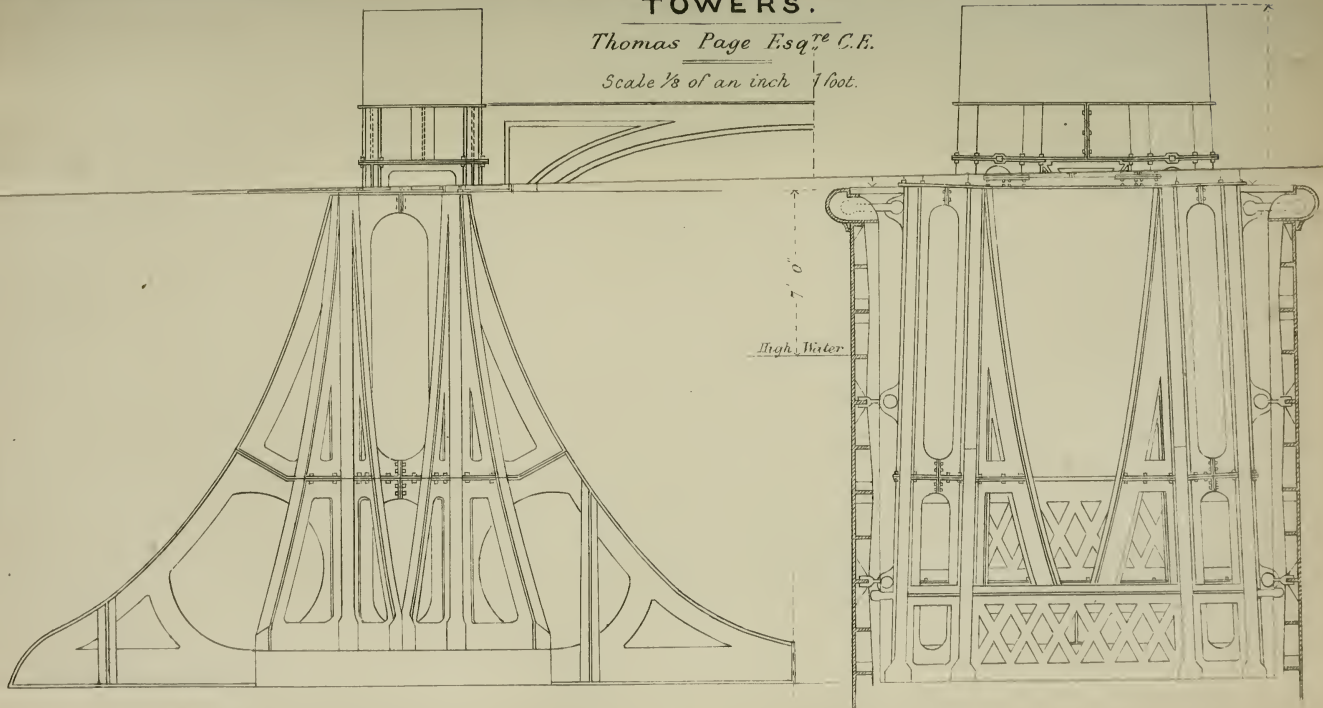
CHELSEA BRIDGE.

TOWERS.

Thomas Page Esq.re C.E.

Scale 1/8 of an inch 1 foot.



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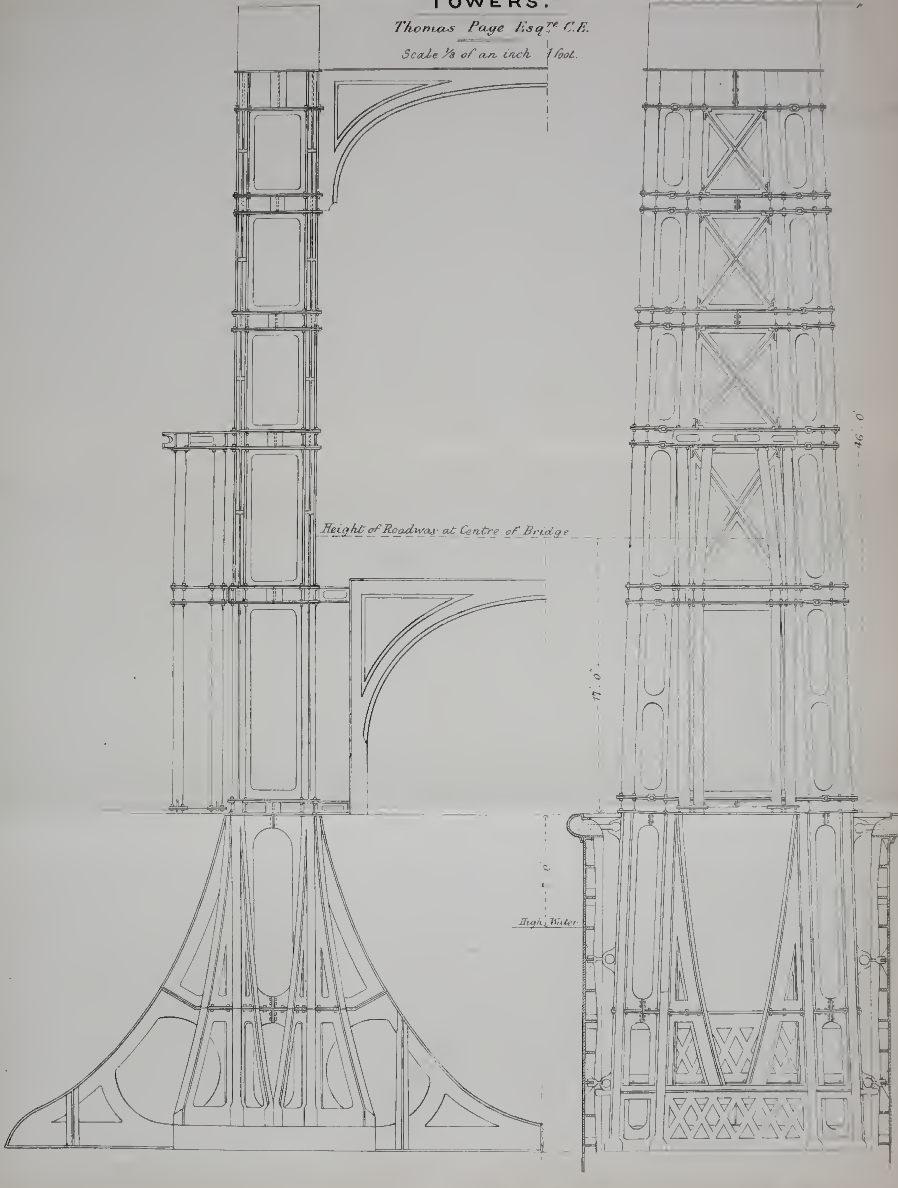
CHELSEA BRIDGE.

TOWERS.

Thomas Page Esq.re C.E.

Scale ⅛ of an inch 1 foot.



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CHELSEA BRIDGE.

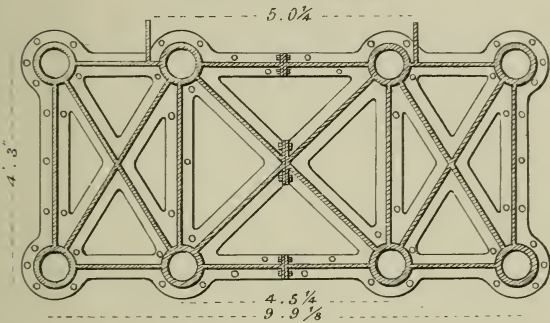
HORIZONTAL FRAMES.

SCALE ¼ OF AN INCH 1 FOOT.

TOP FRAME.

OUTSIDE ELEVATION. INSIDE ELEVATION.

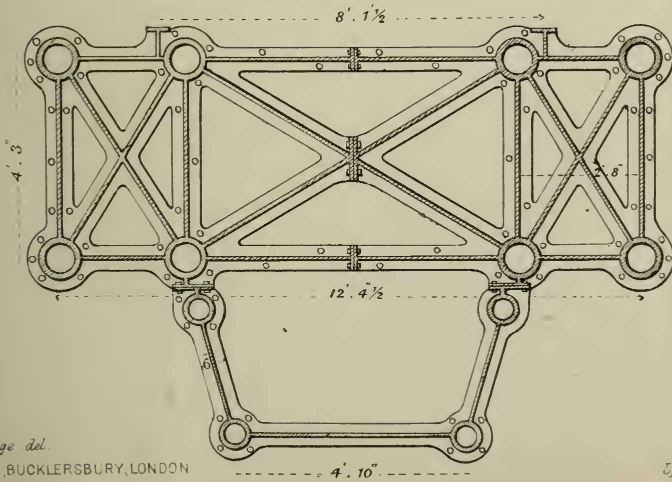
SECTIONAL PLAN.



5TH FRAME.

INSIDE ELEVATION. OUTSIDE ELEVATION.

SECTIONAL PLAN.



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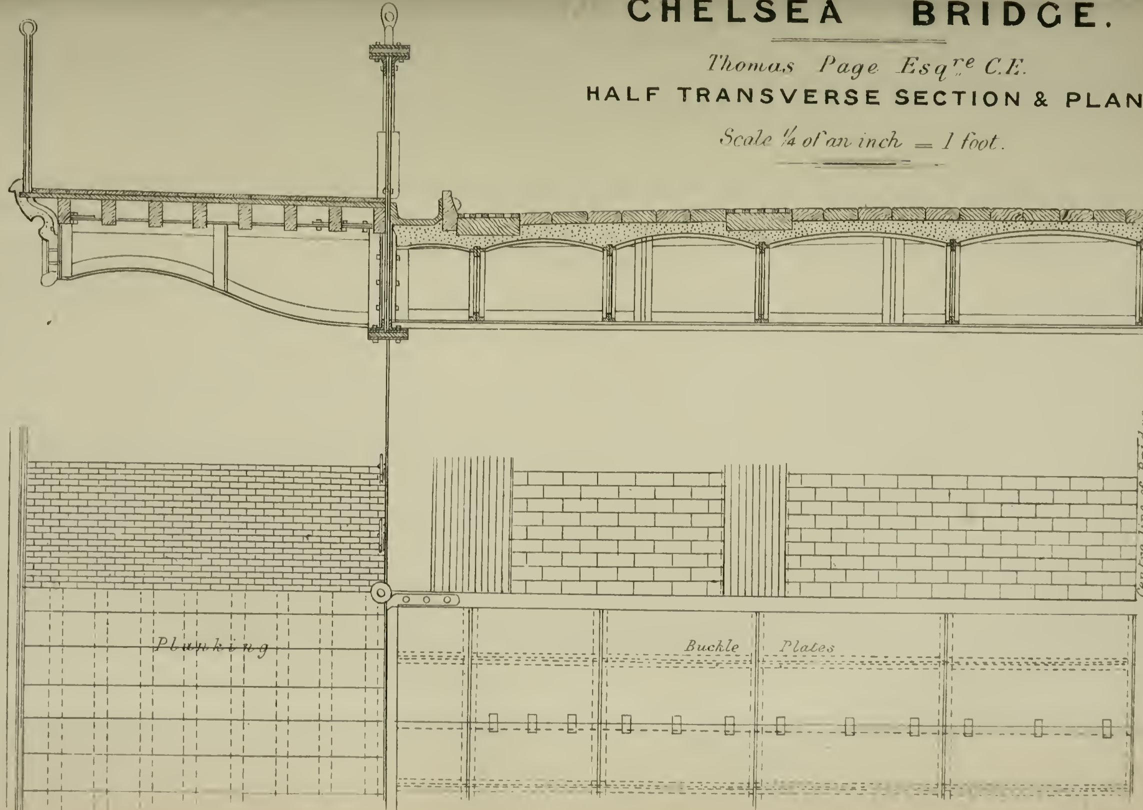
Upfield Green, Litho

CHELSEA BRIDGE.

Thomas Page Esq.re C.E.

HALF TRANSVERSE SECTION & PLAN.

Scale ¼ of an inch = 1 foot.



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F. AND E. N. SPON BUCKLERSBURY LONDON

June 1*st*, 1863.

E. RILEY IN THE CHAIR.

ON THE BOMBAY, BARODA, AND CENTRAL INDIA RAILWAY.

By Charles Sanderson.

(Communicated by W. H. Le Feuvre.)

The late and much lamented Mr. James Berkley having read a paper in 1860, before the Institution of Civil Engineers, upon Indian railways generally, and which, at the time, excited much interesting discussion as to the best mode of conducting railway works in the three Presidencies, it is not the author's intention to do more in this paper than to describe briefly:

First. The general features of the country through which the Bombay, Baroda, and Central India Railway passes.

Second. Some of the characteristics and estimates of the works as designed.

Third. Some of those "makeshifts" which the resident engineer is compelled to adopt in a tropical climate, where time, and difficulty in procuring materials, are elements involved.

Lastly. Some observations as to rolling stock, and traffic working.

First. The Bombay, Baroda, and Central India Railway commences at Bombay Fort, and passing through the Islands of Bombay and Salsette (both of which are connected by causeways), crosses the Bassein Creek, by a bridge of one mile in length, to the mainland of the Tannah Collectorate. It then skirts the coast by Damann, Bulsar, and Surat, to Broach, at a tolerable even distance of only a few miles. Thence, still keeping northward to Baroda (the chief city of the Guikwar), points a little westward to Ahmedabad, a total distance of 310 miles from Bombay.

In its course it crosses the following chief rivers and creeks:

			Length.
Bassein Creek . .	86 spans of 60 feet	. .	5160 feet
Vyturnee River .	41	„ . .	2460 „
Damann River . .	14	„ . .	840 „
Par River . . .	10	„ . .	600 „
Orunga River . .	15	„ . .	900 „
Cavery River . .	21	„ . .	1260 „
Beelemora River .	14	„ . .	840 „
Poorna River . .	19	„ . .	1140 „
Mendola River . .	12	„ . .	720 „
Taptee River . .	30	„ . .	1800 „
Narbudda River .	59	„ . .	3600 „
(Two of the spans are 90 feet)			
Mhye River . . .	27	„ . .	1620 „
Watruck River . .	10	„ . .	600 „

besides seventy-five smaller rivers and nullahs varying from 600 feet and under in width, all crossed by iron screw pile bridges, the total length of which is 6⅛ miles.

In addition to these, there are several brick and stone bridges of 3 spans and less, numbering 155 including 20-feet spans.

All the rivers, with the exception of the Mhye and Watruck, are affected by the daily tides; at the point of crossing, the greatest range of tide is 10 feet, and the maximum known flood-level of the Narbudda is 51 feet above the summer level. The waters of the Narbudda and Taptee attain a velocity of 8 miles an hour, both in monsoon floods and high spring tides.

The creeks southward, and the rivers northward, have a substratum of hard clay and kunker. Whilst the intermediate rivers, such as Damann, Sinjun, Par, and even Orunga, have beds of trap rock, underlying a few feet of alluvial or clay.

It has frequently been observed of this railway, that apart from the numerous rivers crossed, the country admitted of a very easily constructed and cheap line, on account of its extreme flatness. This, however, is by no means true in all respects; for, notwithstanding the fact that the southern part of the line passes over immense flat tracts of land flooded by spring tides; or, that the northern portion traverses many miles of flat cotton soil district, yet, in the former

case, the difficulty of procuring sufficient material to make up the embankments, and in the latter, the porous nature of the soil and the difficulty of getting good foundations, have added largely to the cost of the railway.

The stone which has been obtained from the 10-feet cuttings about Sinjun or Kelvee Mahim, proved to be useless for building purposes. So that quarries had to be opened at several points between the Vyturnee and Dongree (about 130 miles north of Bombay), the cost of getting, dressing, and carriage, being about eight annas (1s.) per cubic foot. Fever generally prevails to a large extent in these quarries, even more so than in the marshy districts. It may be accounted for, in the fact of jungle and timber being adjacent to the quarries.

On the whole, Guzerat is tolerably healthy, and Europeans may live in the flat districts overlying the sea coast with perfect safety, it being simply a question of temperate habits. The amount of rain varies from 100 inches in Bombay to 30 in Surat and 20 in Ahmedabad. The whole falls generally between June and October. No earthworks can be carried on during those four months; but where bridges and other works are accessible by roads, the rains do not offer very serious impediments to bridge works on tidal rivers not subject to monsoon floods.

In the northern rivers, during monsoons, large quantities of sand and organic matter are brought down from the uplands and forced into the Gulf of Cambay. Much of it contributes to alter the beds of rivers; scooping out in some places, and filling up in others. This rendered it necessary that the foundations, whether masonry or screw piles, should go below the sands, although in some cases 30 feet deep. In the Narbudda, many of the piles went 45 feet deep.

The population of the districts through which the line runs, averages 257 to the square mile, with about 13 per cent. of males over females. A large number of the latter were employed upon the railway in forming earthworks, their wages being two to three annas per day. The male coolie earning from four annas to six. The above average of 257 does not include Bombay and the island.

When required to do so, the coolies, both male and female, will work in the rains; but it was only in very pressing cases such work could possibly be profitable.

The superstitious habits of the people often militated against the quick progress of the works; for instance, in forming a temporary bank across the Mhye, in summer time, many of them ran away rather than close up their goddess. The same applied to the work-people of the Narbudda, they could only believe that the bridge could be safely opened when four Europeans had been accidently drowned in the rapid currents of the river, who being thus immolated upon the altar of their favourite goddess, propitiated her.

Second. The surveys of the railway were ordered by the Honourable Court of Directors of the East India Company on the 10th of August, 1853, and sanction was obtained on the 16th of April, 1855, to commence the works of the northern section from Surat to Ahmedabad, a length of 143 miles. The southern portion of the line was not sanctioned until the 20th of November, 1857, and even that did not embrace the last 7 miles of the terminal part into Bombay Harbour. Considerable delay had occurred in the settlement of the southern portion of the line, in consequence of varying opinions as to where the railway should join the Great Indian Peninsula Railway, and whether the terminus should be common to both. In November, of 1861, final sanction was given to the above mentioned 7 miles, and also for an independent terminus; but that arrangement has since been disturbed by the agent of the company, so that at the present moment it is believed that no works for a permanent station are yet commenced.

In December, 1860, the author having been appointed chief engineer in India, proceeded to Bombay ; and in February, presented his first report of the middle portion of the line, between Brelsar and Baroda. The remaining portions of the line, north and south, being reported on in the following month of March. The author found that his predecessor, Mr. Forde, had had great difficulties to contend with; not so much in the construction of works as in the organization of the departmental system. The author found 80 miles open for traffic, between Surat and Baroda, half of which had been open only a few days, on his arrival January 16th, 1861. For many miles the permanent way was laid on the bare cotton soil. The Narbudda bridge not being complete caused a break in the continuous traffic, the river and transhipment of goods requiring one hour and a half

to two hours, besides being highly dangerous in the rapid tidal currents.

By the 12th of March, 9 miles more were opened south of Surat, and on the 7th of April, 9 miles northward of Baroda. A farther opening, southwards, of 34 miles, took place on the 2nd of September, 1861, making a total of 132 miles, or 52 miles during the author's first half-year of service. In addition to this, there was the completion and opening of the Narbudda bridge (3,600 feet in length) on the 20th of June, after the most strenuous exertions of Mr. King (the bridge engineer), and just in time to save the monsoon.

The general works of the line are of the ordinary class, the greatest cutting being 26 feet, and the greatest embankment 29 feet. The gradients have been laid down originally to have nothing sharper than 1 in 500; that, however, is not the ruling gradient, for there are several of 1 in 100. One of these the author would have been glad to have dispensed with, between the Umbudda bridge and Broach station, but for the fact of the expense and the want of time. A proposal was made, in the author's early reports, to adopt, here and there, sharper gradients of 1 in 200, or 1 in 250, with a view to lessen earthworks and lower river bridges, but the proposition met with such strong opposition from the agent and others, that it was abandoned, excepting in some cases, where sanction was obtained to continue falling gradients and lowering the long embankments over flat and flooded districts. The general plan and section (Drawing No. 1), together with the tabular form B, will show the general gradients. With regard to the sharper gradients of 1 in 200 and 1 in 250 (not exceeding three-quarters of a mile in length), the author's agreement was, that momentum alone would have prevented any serious retardation to a maximum load, or drawback to the designs of Colonel Kennedy (the consulting engineer), who contemplated flat gradients and one monster train a day. It was never intended by the author that such sharpened gradients should be placed in a position where a heavy train would be required to start upon them.

The cotton soil of Guzerat makes very bad embankments. Being exceedingly porous, and capable of immense expansion and contraction, such material must ever be a source of anxiety to the engineer,

especially if at all exposed to monsoon floods, in addition to 50 or 60 inches of rain fall. Indeed, without a good covering of turf, babul plants, or other thickly growing shrubs, it cannot stand heavy rain. In one case, about seventeen miles north of Surat, a whole embankment, standing above the ground level about 12 feet, was not able to stand the brunt of a 10-feet flood, for, being composed of the cotton soil, it got soaked and pushed over 1 foot 6 inches (permanent way and all) before the water subsided. The earthworks on the whole of the line have been accomplished by coolie labour with baskets. An experiment was once tried with wheelbarrows, but utterly failed, the coolie always preferring head-work. The ordinary way of executing earthworks, both on our own lines at home and some in India, viz. by cutting and tipped embankment, would certainly have obviated a difficulty arising out of side pits, and which promises to be a very formidable objection to basket-made embankments. In nearly all the cases of rivers having old channels, or blind rivers, the flood waters are apt to take the direction of the side pits (which are excavated parallel to the railway), and wasting in their course the toe of the slopes. A diagram (No. 2) showing the Narbudda River and its obsolete channel, or "blind river," will explain the foregoing remarks. The channel has a 28-feet embankment put over it, instead of a viaduct of fifteen 60-feet spans (as originally proposed by Mr. Forde, the previous engineer), and along both sides of the embankment are the holes excavated for material. Now when the mighty 50-feet floods come trending from the north-west down the valley of the Narbudda, they have not time to turn the corner on the east of the railway, so they naturally take their old course, and plumping up against the embankment, get driven down the side holes on the east side, and rounding the abutment of the bridge, take the lower level again among the side pits on the west side, until the flood reaches the old channel again, proving the necessity of keeping such channels open by viaducts. What is termed the "blind river bank" has not only been a fruitful source of much correspondence, discussion, and delay, but, in a pecuniary point of view, will be costly in maintenance. The dotted lines in the diagram No. 2 show the direction of the flood waters. There is another case of a similar character at Ambulsar (twenty-nine miles south of Surat), where the "blind" part

of the river Umbicka, finding itself suddenly arrested by the railway embankment, took to the side pits, and finally through a 20 feet archway, burrowing a hole 20 feet deep close to the foundations. The argument of the engineers who designed these works is, that once the dam is formed, the water will cease to flow that way, at the same time providing neither breast-walls or puddle to meet the pressure and impetus. On the contrary, the side pits offer a new channel, which in every case seriously wasted the works of the railway. It is true that the consulting engineer endeavoured to meet this contingency by directing that the holes should not join each other, but that partitions should be left. These, however, are soon washed down in the under currents. The author has always been an advocate for proper openings, even at old channels of rivers, and in cases where traffic is actually passing over such embankments, it was proposed to build the usual iron screw bridges, as if for double line, and if nature insisted on the destruction of the embankment, it would soon be accomplished without much labour. Nevertheless, in some of the worst cases it would absolutely pay to refill the side holes with the spare embankment when the bridge is completed. The earthworks throughout the whole line are only formed for single road. North of Baroda the soil is very sandy, and the monsoon rains waste the embankments composed of it very much. From the river Mhye, northwards, most of the earthworks were completed in 1859, and waited over two monsoons for the permanent way. In order to preserve the tops and slopes, "bends" were made, *i.e.*, the tops were converted into little reservoirs of about 1 foot deep, somewhat after the fashion of "paddy bunds." This tended in a great measure to prevent the washing away or serrating of the banks.

In the cuttings, there is a very useful limestone, familiarly known as "kunker." This makes most excellent lime and chunam. Indeed, it appears to have compensated for much loose and careless work. The kunker burnt with wood costs about 11 rupees per cube yard; whilst the inferior sort, burnt with cow-dung, sold at 8 or 9 rupees. The lime is hydraulic, and takes 2 parts of sand.

The fencing used on the line has been various. There is the teak-post and jungle teak-rafters, at about 9 annas (14d.) per yard; also a rougher kind of jungle timber-fence at 6 annas per yard, and a

slight bamboo-fence tied with coir rope, at $4\frac{1}{2}$ annas. The two last have been used only for temporary purposes. The wire-strand fences with teak-posts, have been too inferior to command success. There has generally been an extra side width of land allowed by Government, say 50 feet from the toe of slopes. This would be useful, planted with babul, as that wood after seven years' growth makes excellent fuel for locomotives. Lands outside the allowance of 50 feet, are charged to the railway company at a rental of one rupee per acre.

The natives very frequently break down fences in passing over the line; and not unfrequently, cattle also stray on to the line, and get destroyed by passing trains. The white ant is very destructive to the unseasoned, sappy woods. Tarring and charring is absolutely necessary, even with the better woods used in fencing.

The permanent way of the railway is of three kinds, viz. the ordinary cross-sleeper road, with rails 65 lb. to the yard. 2. The "sandwich" permanent way, invented by Mr. B. W. Adams; and 3. A suspended girder-rail, by the same inventor. Of the two latter kinds of road, there were not more than 40 miles on the line, out of 310 miles. The sleepers for the ordinary road are creosoted Baltic timber, at a cost, delivered on the ground, of 10s. each. There were enough to lay 60 miles of road. But a considerable portion of them proved so useful in the staging for iron-bridge erection, that some 30,000 of them got absorbed in that way. The remaining supply of sleepers was of teak, sadra, khair, or pine. The teak costing about 4 rupees, and the inferior woods 3 rupees per sleeper. The creosote and the vibration of passing trains together, entirely prevented the ravages of the white ant. Sometimes when native sleepers, or even Malabar teak, remained piled long together on the bare ground, the white ant was sure to have attacked them largely.

With regard to the Adams's "sandwich" road, it was too light for embankments, with only a limited supply of ballast and heavy 50-ton engines. The cross-ties, 5×5 creosoted timber, broke by dozens. The principal objection to the introduction of that class of road, was the difficulty of grooving and boring the longitudinal timbers, the native woods spoiling the tools sent out, from sheer hardness. The author was compelled to send both the "sandwich"

road and the suspended girder-rail into cuttings, where a good firm base could be obtained. When the "sandwich" road was nicely packed and settled, the smoothness of the running was very remarkable at speeds under 20 miles an hours.

The suspended girder-rail was laid for about 4 miles on the Narbudda embankment, and was altogether too weak for heavy traffic. The speed was always slackened over that portion of the line.

There can be no doubt but that the double-headed rail is the best for India. It can be turned four times, and even then be useful in sidings or light branch traffic. The rail for the "sandwich" road is 6 inches deep and light in the web, and in the absence of longitudinal sleepers, the author cast a few suspension chairs (Ordish's patent), and adapted them very successfully. It will be an economical plan to absorb broken piles in this way, if it should be found that the suspension chairs answer with so great a depth of rail.

The ballast question of the northern portion of the railway was one of very difficult solution. North of Surat, there was nothing to be had but the sand from the beds of rivers, or moorum, which underlaid the black cotton soil, and often got largely mixed with it. So long as the dry weather lasted, the packing might have been done with the sun-hardened lumps of cotton soil; but no sooner had the rain penetrated the ground, than the whole road dropped into disorder, and often stopping the traffic for hours. Indeed, so great were the difficulties and anxieties involved in attempting to work any portion of line without ballast during the rains, that the author was strongly opposed to the repetition of premature openings similar to that of Broach to Baroda, 44 miles, 30 of which were unballasted, and the opening of which occurred on the 9th of January, in the absence of a chief engineer.

The bridges of the line, as before stated, are nearly all built on Mitchell's screw-pile system, with Warren girders. This class of bridge is so well known to the engineering world, that it will only be necessary to give a few details explanatory of the accompanying diagrams. Nos. 3 and 4.

The diameter of the cylindrical piles is 2 feet 6 inches, the thickness of metal 1 inch. Length of piles uniform, and 9 feet, weight averaging $1\frac{1}{2}$ tons. With the exception of the bottom piles (which have

their flanges inwards), the flanges are all on the outside, and held together with twelve 1-in. bolts. The fracture of piles generally occurred at the flanges, during the progress of screwing. Large 40-feet bullock arms have been tried for screwing; but it was found so difficult a matter to control the strain, that the old method of capstans and coolies was resorted to. The staging required for the erection of the three-piled tiers, was generally 50 feet wide, composed of jungle teak round timbers, averaging 20 inches girth. The decking of the staging being composed of European creosoted sleepers, which afterwards came in for the permanent way.

The average rate of actual screwing per day was 1 foot, at a cost of 43 rupees in deep rivers to 15 rupees in dry beds; 5-inch rope (Manilla) was used with the capstans, which were adapted for the use of double-headed rails inserted into the cap. The spokes of the screwing-collar were also pieces of double-headed rail. A very large amount of Manilla rope was used, in some cases, a ton to a span.

The piles were screwed for a double line, but the girders erected for a single one only. The spans are 60 feet. Depth of girders, 7 feet 8 inches. Width of compression beam at top, 1 foot 5 inches. Thickness of compression-beam plates, 1 inch. Thickness of tension bars, $\frac{1}{2}$ inch. These are two in number in the centre bags, and increasing to 6 at the ends. It was originally intended that the compression beams (which were shipped in three parts), should be bolted together with the lap-plates with $1\frac{1}{4}$ bolts; but riveting was found more complete and secure.

The tiers of three piles were originally intended to have strut piles of the same dimensions, both up and down stream, of all important bridges; but in practice they have not been applied (excepting on the Taptee and partially on the Narbudda) on both sides. It was found difficult to screw them at an angle of 35 deg., and, indeed, they were not necessary, excepting on the down stream.

The author, in his early report, recommended the use of timber fenders instead of the up-stream strut piles, in the case of the Narbudda. There would be a danger of piles being fractured by floating timber or native pattimars, in rapid, tidal, or flood currents on that river. Sanction was given to put these fenders in, and 30 were completed by October of last year. They were composed of teak

timbers 14×14 and shod. They raked up at a batter of 1 foot in 20, and were tarred. The tiers of piles were braced together by horizontal T and diagonal L bracing. No longitudinal bracing or ties were used, excepting some $1\frac{1}{4}$-inch rod, connecting the tops of the unused piles, as they were observed to oscillate during very high floods.

It was never intended that there should be decking on the bridges, only side planking for mere gangways. The longitudinal teak balks, carrying the permanent way, were bolted down to the wrought-iron cross-girders, which occurred at each standard, or 7 feet 6 in. apart. The author having witnessed the effects of a serious accident, when he had just arrived in the country, felt the necessity of having either decking or some kind of fender, to guard the double-headed rail. accordingly asked for 7-inch decking, and (that being refused) subsequently for 4-inch, also for timber fenders or curb, as a stop to the decking. These arrangements might even prevent an engine from doing serious damage to a Warren girder, and certainly would be sufficient to check a carriage when getting off the rails. In the case of the Wishwametree-bridge accident, a native, or natives, having a design against the life of the Guikwar of Baroda, placed a double-headed rail across the line, only a few yards from the bridge. The train was running about fifteen miles an hour, and was thrown off the rails. The engine and tender bounded from one cross-girder to another in a singular succession of jumps, finally hanging by the axles on the permanent way which was severed from the bridge. If that permanent way had not been fished the whole train must have fallen fifty feet below. As it happened the Guikwar was not in the train, and the passengers escaped by jumping on to the compression beam.

In addition to the decking and fenders asked for, the author indented for flat-bottom bridge rails, as being safer and better adapted for bridges. Unfortunately those rails are not yet on the line, and the ordinary rail and chair continue to be used.

Drawing (No. 3) shows the construction of the 60-feet girders, together with sizes and weight of the parts. No other spans have been used on the line except the "makeshift" girders, which will be described hereafter.

The stations of the railways are for the most part temporary or "cutcha." Only three had been completed in brick, viz. at Broach, Unklesur, and Keem. The bricks are burnt in kilns with babul wood, and cost from 15 to 20 rupees per thousand. The temporary stations are simply upright jungle-teak posts (or sotas), filled in either with brick nogging or lath and plaster. When the author left India several of the northern stations had been commenced with brick, but which would require an outer surface of chunam. Materials were difficult to procure, on account of long distances and no roads—so that the most temporary stations were expensive.

Platforms, paving, and nosing was done with Gogo stone, from the other side of the Gulf of Cambay.

There are some very good specimens of brick bridges on the northern part of the line. The Mhaynee, of 3 arches, for instance, with 40-feet spans, and 40 feet high, looks very well. But many of the single 20-feet brick arches and culverts are cracked at the crown. The common-sense conclusion was, that for want of backing and counter forts the abutments had followed the direction of the shrinking earthwork. The popular cause was, however, assigned to an earthquake. Whether there is, or is not, any ground for this assertion, there are large temples at Ahmedabad rent from top to bottom by earthquakes of recent date.

The "Swindon" of the railway is at Amrolee, on the north bank of the river Taptee, and about two miles from Surat. The position is not considered a healthy one, partly from being surrounded by native villages, and partly from native bodies being burnt on the opposite bank.

The engine-repairing shed is small and only temporary. The carriage building is conducted on a large scale—the iron work being obtained from England. A small foundry is at work, also vertical and circular saws. The permanent workshops were designed and submitted for sanction in the autumn of 1861. The locality is proposed to be forty-three miles south of Surat, at a place called Bulsar, where the sea breeze has full play. Those works are to cost 50,000*l.*, and are now about being commenced. One great advantage of the locality is the closeness to a river and the sea. Water carriage is

very useful and economical where the transit of an engine over 100 miles of ground to its destination is concerned.

Turning now to the third point, viz. the "makeshifts" of an Indian railway while under construction, the author is reminded of a maxim of a late and much esteemed president of the Institution of Civil Engineers—Mr. Robert Stephenson—that he who could tell of some failures as well as successes in engineering, was the greatest benefactor to the Institution. The screw-pile bridge with facility of erection is certainly a success in the alluvial rivers of India. The failure of much masonry, both brick and stone, along the whole course of the line, has proved beyond doubt that the cotton soil, clay and moorum, are, at the best, unsafe materials to deal with, and so far the screw pile has been the only remedy for such a country.

The Narbudda bridge has been very difficult of erection, even after the engineers had had the experience of the Taptee. In 1859, a high spring tide got such a strong hold upon the staging of the Narbudda, that it broke off short many of the iron piles, and the whole was carried away. The consequence was, when Mr. King, the principal bridge engineer, took to the works, he found that many pile stumps, some 30 or 40 feet down, must be extracted before the sites would be available for fresh screwing. In order to effect this, European divers, with Heincke's patent dress, were employed; but the work was so tedious that the author had to devise a "makeshift" in order to open before the monsoon burst upon him in 1861. That "makeshift" consisted in adapting such 60-feet iron bridge parts, as were in the country, to make up 90-feet spans; the increased depth of the compression beam by 8 inches; tension bars increased in number from 2 to 4 in the middle, and from 6 to 12 at the end. Also the substitution of $2\frac{1}{2}$ in. main pins, instead of $2\frac{1}{4}$ in., enabled the author to get a safe girder, capable of bearing a rolling load of 144 tons with only $\frac{3}{4}$ inch deflection. Sanction was obtained from government to erect the spans in two places on the bridge. The home authorities were written to as to the alternative which had been adopted, and only just before the burst of the monsoon in June, was that difficult bridge open in time to convey troops northward.

The suggestions from the board at home to put 45-feet spans, or a

combination of 15 ft., 60 ft., and 15 ft. unfortunately came too late, for they arrived only six days after the bridge had been opened. It is intended, of course, to restore the 60-ft. members to their proper sphere, and send out new 90-ft. girders from England. The bridge has now been opened sixteen months.

Another "makeshift" adopted was that of increasing the base of the screw piles by building round them 5-feet wells, and filling them with concrete. Persons unacquainted with large Indian rivers, have little conception of what immense changes occur in the beds during one monsoon. Foundations of bridges are often undermined where least expected. For instance, the Mhaynee bridge of three arches, before alluded to, had its foundations laid bare; while the Keem bridge, with a 60-ft. Warren girder and brick abutments, had ten cubic yards of masonry washed clean out of the foundation, causing the abutment to crack and heel over. This latter case was remedied by making it into two spans of 60-feet iron piles.

The wells at the Mhye River (see drawing No. 4), were adopted on two grounds. First, that they afforded a better base than 2 ft. 6 in., and where, with a height of 100 feet above the bed of river, and a flood rise of 54 feet, it seemed highly desirable to get something more than the mere 2-ft. pile, as was designed for it in England; and second, the sinking of wells much cheapened the cost. While screwing cost 20 rupees per foot, sinking in wells came to about 4 rupees per foot. The wells were simply sunk in the ordinary way. The objection to using lime arose out of the fact of the divers getting it into their eyes. When the pile was screwed into the bottom of the well, to the requisite depth, both the pile and the well were filled up with concrete, and the latter domed over with brick-work and chunam. These wells were sunk only at 26 places at the Mhye bridge, instead of 100, owing to the consulting engineer having put a stop to them under an impression that they were to serve as *bases* for piles instead of mere casings. Out of the 517 spans of 60 feet, at least half of them have their piles driven into the dry sand or clay, and such a method of sinking, would have been a great saving in construction, when contrasted with the enormous wear and tear of the capstan, involving as it did 5 per cent. of piles in destruction.

The cost of screwing in the Narbudda River (taking the part covered by water) was as follows:

	Average per foot. Rupees.		
Screwing at the rate of 1½ feet per day for each capstan.	25	5	0
Cost of extracting broken piles and diving (Heincke's dress and Europeans)	15	8	0
Cost of broken piles per foot of screwing . .	1	2	0
Unwatering by leathern buckets	1	14	0
	43	13	0

This amount may appear heavy, but there were no pumps to unwater the piles. Although often indented for, it is unfortunate that such useful auxiliaries never found their way on to the line.

The diving department was a very expensive one. The dresses and pumps were costly, and require great care in preserving them from injury by climate and use. The men suffered very much by the air being heated and tasting of oil. The author hit on an expedient of building cells containing saltpetre round the pump cylinders, and this greatly relieved the divers. The expansion of the Taptee bridge girders was only 10-16ths of an inch in 1890 feet. There was no corrosion of the pile cylinders where salt water came into contact with the concreted piles.

Amongst the other "makeshifts" adopted, there were those of using Adams's rails to build portions of the machine shops, fractured screw piles to build engine-lifts, and common railway spikes to make rivets for the iron bridges. Fortunately the latter were of good material, or the author would not have permitted the continuance of a practice which had been in vogue some time before he arrived. It was either stopping the works or using the spikes, for native iron would not do.

Fourth. The rolling-stock question is one about which a few remarks may not be inopportune. On the Bombay and Baroda Railway there are at present 18 engines; 14 of these were working the 132 miles when the author left in February last; 4 others (tank engines) had been sent out and erected subsequently. Out of the 14 engines,

10 are coupled goods engines (4 wheels out of the 6 coupled), 16-inch cylinders, 22-inch stroke, and 5-feet driving-wheel. The average speed (according to the time-table), 17 miles an hour; weight of engine, light, 30 tons; loaded, nearly 34 tons; tender, 13 tons; loaded, 19½ tons; total weight of engine and tender, loaded, 53½ tons. The tractive power at the rails, 6000 lb. The small tank engines built by England were used for ballasting. Their water capacity was sufficient to run only 9 miles. They had 4-feet driving-wheels, 17-inch stroke, and 14-inch cylinders. The larger engines were built by Neilson, of Glasgow. Wood fuel had been used to a great extent, so much so, in fact, as to have cleared the surrounding country of babul wood. About 7 years' growth will give a circumference to the stem of the babul of about 15 or 19 inches. It will not bear to be kept long after cutting, as the white ant gets into it and soon reduces it to powder. The cost of this, cut up in 12-inch lengths, and delivered to the railway, is about 13 rupees per 100 maunds of 37½ lb., or 26s. per 33 cwt. The quantity of this used with the large engines was about 80 lb. per mile (running at 17 miles an hour). Coal from Newcastle has been bought in large quantities in Bombay Harbour at 16 rupees up to 20 rupees per ton, but delivered on to the line at Surat would be more like 50s. per ton, or 25 rupees. The quantity of this (used with coke) was about 30 lb. per mile as compared with the wood, or at a cost per mile of ⅐th more for coal. The wood fuel of India contrasts favourably with that of America, the latter costing, according to Mr. Zerah Colburn, 9d., and the Bombay, Barodo, and Central India Railway only 4½d. per train mile. The babul wood is much denser, and throws out a stronger heat than the American woods.

The average load of trains (exclusive of engine) is 150 tons on the Bombay and Baroda Railway, generally consisting of an average number of 22 carriages and goods waggons. The ballast waggons of the line were often used to get the ordinary traffic out of hand. Cotton waggons, capable of holding 20 bales, were much needed, but notwithstanding much had been said and designed on the subject, not waggons had been placed on the line worth speaking of when the author left, so that cotton bales, after lying at the up-country stations, were obliged to go by ordinary road. The bale, as pressed

in the Mengaum or Surat districts, averaged 4 cwt., and the size was 4 feet 9 inches × 4 feet × 3 feet. The pressing was by no means as complete as it might have been. The usual plan was to half press them up country, and complete them in Bombay.

The question of third-class carriages had been occupying the authorities in India. The result of the frequent discussions was a carriage with two floors, not requiring that any passengers should squat, but that there should be accommodation for sitting in both upper and lower floors, the passengers on the former placing their feet in a trough-like space, where the lamps usually are. The cost was about two-thirds that of the ordinary teak carriages sent out from England. The natives had a great dislike to the squatting on floors, especially the Parsee class, so that the original carriage with two floors and bars like a sheep-truck remained unused in a siding.

The traffic of the line averaged 7*l.* per mile per week. This no doubt would have been much more between two great cities, Baroda and Surat, had there been sufficient rolling stock, for Baroda is the centre of a thriving kingdom under the Guikwar, and Surat a flourishing port, with at least a dozen steamers in almost daily communication with Bombay. When the line is opened southwards to Bombay, and northwards to the thriving city of Ahmedabad, and with a sufficiency of rolling stock, the receipts will no doubt reach those of the Great Indian Peninsular Railway, say 20*l.* per mile per week. This, on 13,000*l.* per mile, and working expenses at 55 per cent., would give 4 per cent. dividend. There will always be a large amount of competition by sea.

In January last (the height of the season) the goods traffic was $\frac{1}{6}$th of the whole, and even this consisted principally of railway materials. The Baroda and Surat length of 80 miles had been open a full year. This result was due to the want of rolling stock and circumlocution combined, for there are all the elements for as good a traffic, at least, as the eastern lines opened only between provincial cities.

The per-centage of receipts in the passenger traffic for January last was:

1st Class, 3.81; 2nd Class, 7.09; 3rd Class, 89.10.

The comparison with other lines in India for the same month was as follows:

East Indian (Howrah to Burdwan, 320 miles), 45*l.* per mile per week.

East Indian (Allahabad to Cawnpore, 24 miles), 14*l.* 10s. per mile per week.

Great Indian Peninsular Railway (part open to Bombay, 437 miles), 12*l.* 10s. per mile per week.

Bombay and Baroda (Bulsar to Dolia, 132½ miles), 7*l.* per mile per week.

The third-class accommodation, it will be seen, is of the utmost importance, the traffic from which forming, as it does, 90 per cent. of the passenger traffic. Longer platforms, pure water laid on for the convenience of travellers, and shelter from heat and rain, together with comfortable carriages, are points for attention and consideration.

DISCUSSION.

Mr. Le Feuvre begged permission to make a few remarks before the discussion commenced. He, Mr. Le Feuvre, had taken upon himself the duty of reading this paper from the fact that he knew Mr. Sanderson, and had been requested by him to bring it before the Society. When Mr. Sanderson wrote this paper, it was with the view that he should read it himself, but he, Mr. Le Feuvre, need hardly say that was impracticable, as Mr. Sanderson was still in India. He might also mention that the paper was entirely due to Mr. Sanderson, and that the whole of the diagrams were drawn by him.

Mr. C. G. Wilson said there could be no doubt as to the interesting character of the paper, and he hoped some members of the Society would be able to give some important facts as to the costs of the line.

Mr. Wells said during the time he was in Bombay, Colonel Kennedy wished him to go to Surat to give Mr. Sanderson some advice relative to the screw piles, but his stay was so short in Bombay that he had not time to do so. The system adopted was the ordinary one, viz. that of screwing down the pile as far as possible, and then by taking out the core the pile was readily fixed to any depth. In some cases they were screwed down as far as 35 to 40 feet, according to the nature of the ground. He had at the present time one of the assistant engineers fixing similar piles in Oxford, and if business had permitted he would have attended and given every information necessary. As to the system of screw piles, that was so well known as

not to require any remarks from him. All he could say was that they had been entirely successful in India, and there had been no bridges erected in India of such length or so rapidly as those where screw piles had been used.

Mr. C. L. Light had used screw piles very extensively, and as to their breaking at the flange, he could say that he had never known an instance of the kind, and on the Bahia and San Francisco Railway he had used piles of 10 and 12 inches diameter, with 1½ inch flange. The bolt-holes should be all properly drilled. He did not recollect a single mishap where the faces of the flanges were faced, and holes drilled and equi-distant from each other. If occasionally a pile broke, it arose from bad casting. There was one point in reference to these piles that he did not consider at all correct, and that was *casting the lugs on* for the bracings. He had generally used wrought iron clips, which could be attached at any point, and made much better work. He had bought good screw piles in very large quantities, faced, drilled, and delivered for 6*l*. per ton. The piles he referred to were but 2-feet piles, which were quite sufficient to carry a 60 feet span.

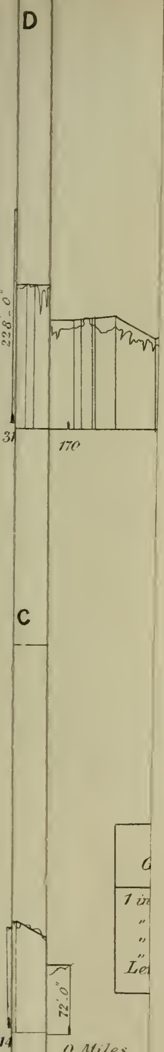
Mr. Wells agreed with Mr. Light that all the flanges ought to be faced. All the screws used for the Bahia railway might have been made of good iron, but those for the Bombay line were so bad that if a hammer were dropped on some of them, they would break, and this was owing to the iron being run from the furnace instead of the cupola. Engineers, he was sorry to say, would not give more than 5*l*. to 6*l*. per ton for the piles, at which price good iron could not be expected.

Mr. Parsey said the paper presented many points of interest as matters of evidence that occurred during the construction of the line. He did not see there was anything novel in the permanent way. The section of the rail was very light for a line of that nature, although no complaint was mentioned; yet it appeared that where the Adams's rail was laid down it shook when a train was passing at 17 or 20 miles an hour. Screw piles and light girders for the bridges over the numerous rivers was no doubt a judicious plan, if properly executed; but there appeared to be several matters in connection with the mode of carrying out the designs that were not so satisfactory. The size of

the connecting pins in the girders he thought were very small for the spans.

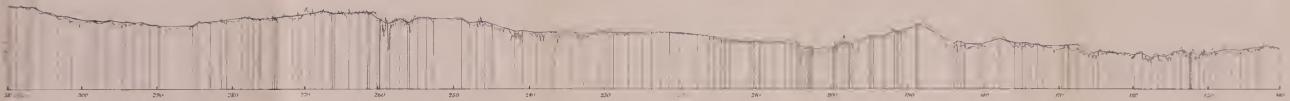
Mr. Le Feuvre regretted that the discussion had been somewhat limited, but in answer to the points raised he might mention that it was originally intended to have a 60-feet span, but it was afterwards arranged to have a 90-feet span, and that the depth the piles were driven into the ground was, upon an average, about 30 feet. As to the breaking of the screws the average was about 5 per cent. Mr. Sanderson's argument was not so much against screw piles as to the difficulty of sinking them in India. As regards the breaking, there was no doubt if a screw pile broke it would be at the flange: these flanges were generally broken in screwing the piles in, several lugs were cast on, which were not used, some had four lugs where only two were required. The piles Mr. Light referred to were only 11 in. in diameter. Mr. Sanderson had to deal with piles 2 ft. 6 in. in diameter. As regarded the boring he thought the plan adopted by Mr. Sanderson was a good one, as it prevented the shifting of sands and the material being washed away. He thought now that the line had been completed the side pits should be fitted up, the objection to doing so before was probably the expense of carriage. As regards the failure of Mr. Brydges Adams's permanent way, he Mr. Le Feuvre mentioned the matter to Mr. Bridges Adams, and it was only due to that gentleman to state that he accounted for the failure by not having sufficient material. He, Mr. Le Feuvre, thought Mr. Brydges Adams's system of permanent way altogether a fallacy. As regards a 60 lb. rail, he thought they would have to renew the line with a heavier rail. He quite agreed with the opinion expressed, that where a Warren girder was used, the load should be upon the top.

The Chairman said he was sorry there were not more present who had been in India, so as to have furnished the Society with some further details in connexion with the system carried out in the different railways. It seemed that the screw piles were a great thing for Indian railways, for without them there would have been very great difficulties in constructing the lines arising from the peculiar nature of the soil. It appeared to be the general opinion that the rails were a great deal too light.

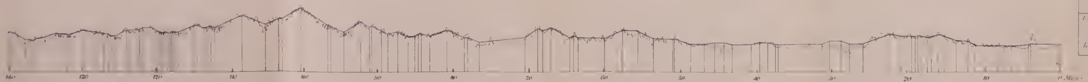


ld Green

BOMBAY, BARODA AND CENTRAL INDIA RAILWAY.



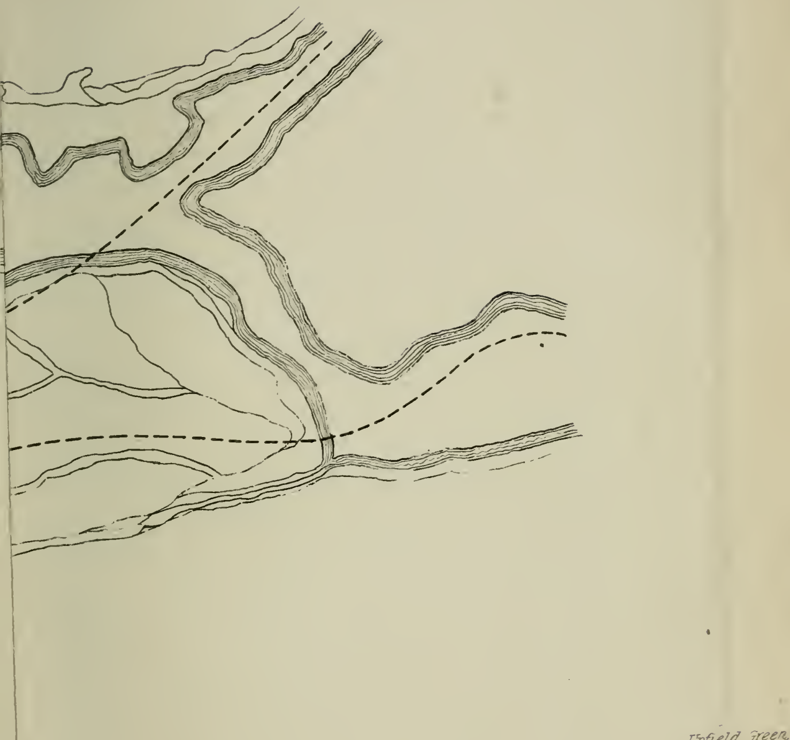
SECTION OF LINE.

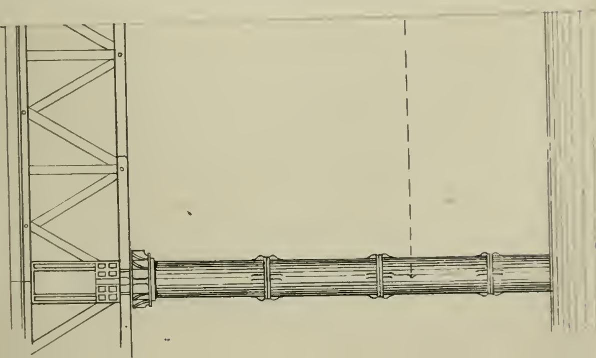


Gradients	No	Aggregate Length Miles ch.	[illegible]	[illegible]	[illegible]
1 in 100	6	[illegible]	0 [illegible]	[illegible]	[illegible]
400 & upwards	101	123 [illegible]	4 [illegible]	[illegible]	[illegible]
700	10	10 [illegible]	3 [illegible]	[illegible]	[illegible]
[illegible]	62	62 [illegible]	3 [illegible]	[illegible]	[illegible]
Level	86	105 [illegible]	[illegible]	[illegible]	[illegible]

AILWAY.

Nº 2.





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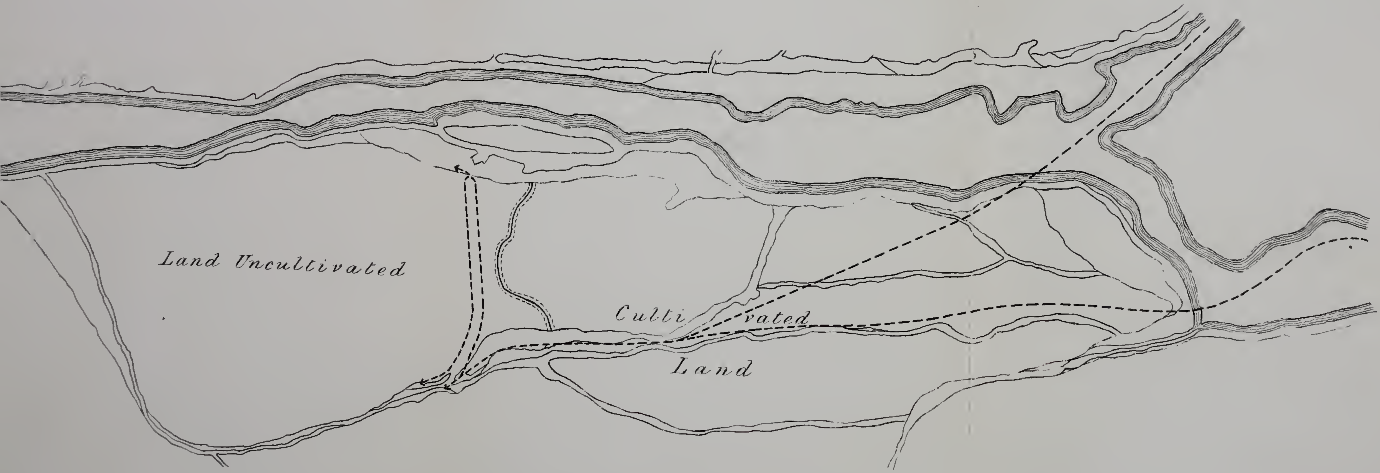
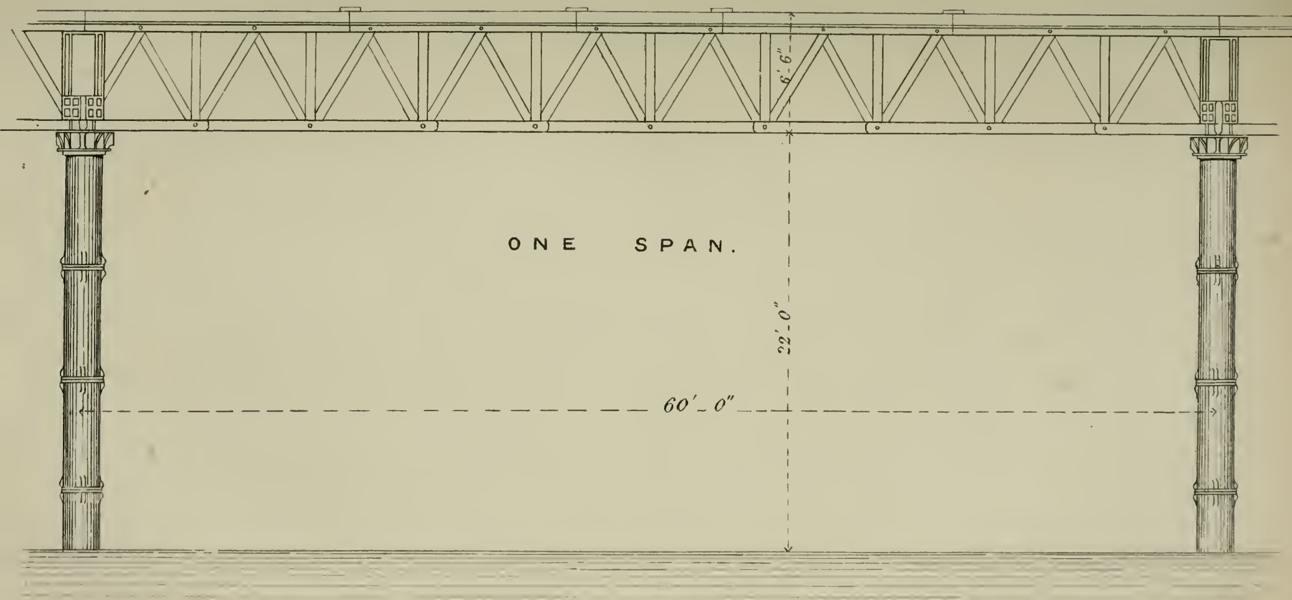


E AND F N. SPON, BUCKLERSBURY LONDON

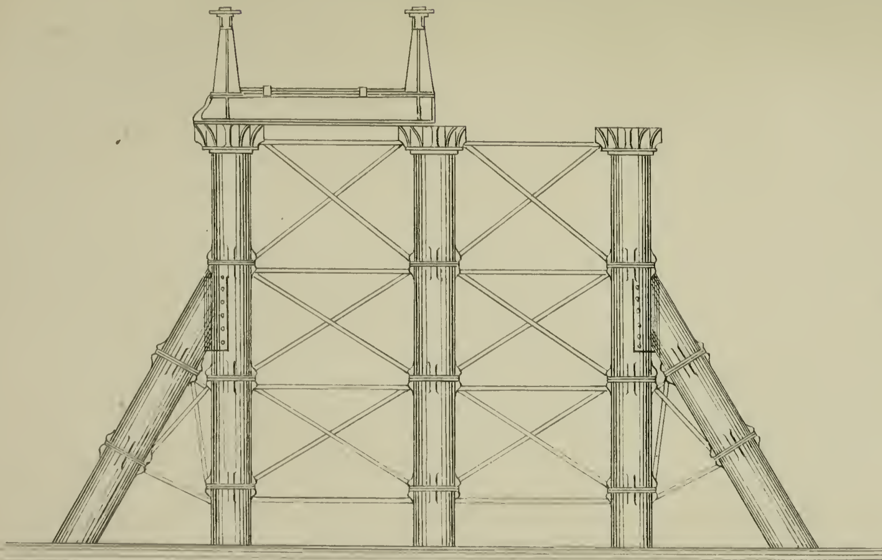
WARREN GIRDER BRIDGE.



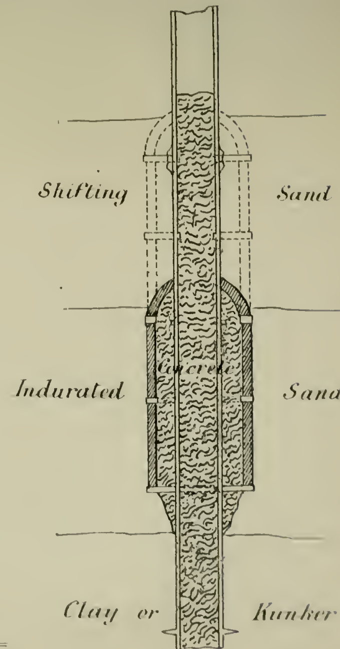
BOMBAY, BARODA AND CENTRAL INDIA RAILWAY. No. 1.

WARREN GIRDER BRIDGE.

TRANSVERSE ELEVATION.



SECTION OF PILE.



F AND F N SPON, BUCKLERSBURY LONDON

September 7th, 1863.

G. W. ALLAN IN THE CHAIR.

ON SIGNALLING FOR LAND AND NAVAL PURPOSES.

BY FRANCIS WISE.

THE subject of signalling from one position to another where circumstances did not permit of a permanent connexion by conducting wires between the points, although most vital in importance to our naval and military services, was evidently one well deserving the attention of the engineer, inasmuch as there were frequent cases in which it would be highly advantageous to him to have the means of communicating with ease and rapidity to distant positions, and of receiving communications from them. The author might instance especially the case in which several parties were engaged on the survey of a large tract of uncivilised country, and who by their isolation from each other, were likely to be frequently placed in circumstances of difficulty and danger, which such means of communication would more or less enable them to avoid. Not only was this so, but those conversant with surveying operations would, the author thought, agree with him in believing that in cases of that kind, a considerable economy of time would also result from a simple and well-arranged system of signalling. There were numerous other cases in which the engineer would find such a system of the greatest service, but it was unnecessary to do more than indicate the fact, and leave those who might have the opportunity, to consider its applicability to operations in which they might happen to be engaged.

The use of signalling from one moveable position to another had hitherto been so much, the author might say almost entirely, confined to the naval and military services, that in treating the subject he was of necessity compelled to refer principally to them, although he looked forward to the time when it should be much better known, and its importance more generally appreciated among the members

of the engineering profession, than it was at present. He trusted also that those present would bear in mind that he in common with many of his brethren in the profession, had had little opportunity of practically learning the facts in connexion with the subject, and would therefore treat with something like mercy, the rather extensive drafts he had been compelled to make on the information and experience of others. If the members would only give him credit for a wish (however ill-carried out), to direct their attention to what he believed to be an important subject for them, and would accept the little information he was able to afford, regarding it as an incentive to further inquiry, his end would have been served, and he should receive the criticisms which his temerity might call down, without complaint, and in a proper spirit of resignation.

The subject of signalling had been well and ably treated by his friend Lieutenant Colomb, of the Royal Navy, to whose researches he was indebted for the majority of the facts referred to in his paper, and who had kindly allowed him the use of numerous highly interesting illustrative models which elucidated the various systems. In treating the historical part of his subject, he should endeavour to occupy as little time as possible, and merely touch upon some of the most salient points in connection with it.

More than 2000 years ago, the importance of the subject of Military signals, was seen and maintained by a Greek historian, who was himself the inventor of a system stated by competent authorities to be of considerable value. At the period referred to, Naval signals appeared to have been of the simplest possible character, and to have been made by a red flag or a brazen shield, hung out from the admiral's galley, and moved in various directions to indicate the movements to be made by the squadrons under his orders. This was, beyond doubt, a very imperfect plan, but it must be remembered, that at that time, vessels were so small, and from their mode of propulsion were so readily manageable, that it was a matter of comparatively little difficulty to convey verbal orders from one vessel to another throughout an entire fleet. Taking this into consideration, and bearing in mind the limited range at which they were effective in their assaults upon each other, it could readily be understood how the fleets of those days were manœuvred by such simple means.

The greater difficulties, arising from accidents of ground, &c., which had at that time to be encountered in the land service, led, as was usual in most cases where difficulties presented themselves, to the invention for it of more perfect arrangements, and caused that service at this period to be considerably in advance of the other, in the matter of signalling. As, however, the vessels increased in size, and propulsion by oars was abandoned, it became necessary to make use of means entirely independent of verbal communication, for conveying orders or information; and about James the Second's time, coloured flags were beginning to be employed for the purpose, without, however, taking a systematic form until probably a century later.

The author would not enter into all the details of the various stages of progress in signalling, but would at once come down to the period at which the art might be considered as possessing a practical value to ourselves; merely remarking that Lord Howe, on the celebrated 1st of June, had only the means of making one hundred and eighty-three signals, and that four years later, it had progressed so that flag ships were able to make three hundred and ten signals. At the present time, the vessels of the Royal Navy could display about fourteen thousand distinct signals, and those of the merchant service seventy thousand.

It was thus seen that Naval signalling had made important progress. The same could not, however, be said of signalling in the sister service, where, although its importance was obvious, there had been until lately a very manifest want of some simple system which would enable commanding officers to rapidly and with certainty communicate their orders to those under their command, and thus to make the movements required at any particular time, without trusting to the transmission of orders by word of mouth, and taking all the consequent chances of delay and miscarriage

The author did not propose in the confined space of this paper to touch upon all the systems of signals that had been proposed; for their name was legion, and a vast number of them were evidently so well deserving of the obscurity in which they had remained, that it would neither be pleasant nor profitable to occupy the time of the Society in detailing them.

He might, however, briefly refer to one or two, which, if not

thoroughly practical in character, had at least some redeeming features either in the way of mechanical ingenuity, or of arrangement indicative of improved ideas upon the subject.

Thus, in the year 1801, a patent was taken out by James Boaz, a manufacturer of Glasgow, in which an attempt was made, by the use of mechanical contrivances, to render the making of signals less liable to error than it had hitherto been. This inventor did not seem to have had the idea of any code or system of signals; but to have depended upon the spelling of the words he wished to communicate, letter by letter, not merely by symbolling each letter; but by actually producing the form of the letter itself. His contrivances, shown in diagrams 1 and 2, displayed considerable ingenuity, and consisted, for night signalling, of a frame or case, containing twenty-five lamps in the form of a square, and each capable of being obscured when necessary by a cover, which, when at liberty, fell over the orifice, through which the lamp was displayed. These covers were connected by cords or chains, with a series of boards working (at each end) on pivots, and arranged beneath the lamp-frame. Now, presuming that it was required to signal the letter H. In order to do this, a certain number of the lamps had to be obscured, leaving only those visible which would convey to the observer the idea of that letter, as shown in the diagram. For this purpose the inventor made use of a slide marked H, and provided with certain projections which, when the slide was pushed in, came in contact with so many of the boards referred to, as were connected with the covers of the lamps required to be exposed, thus instantly giving them a tilt, drawing down the required cords or chains, and simultaneously exposing the whole of the lamps necessary. Similar slides were provided for signalling the other letters; and numbers, &c., if required.

For day signalling Boaz proposed to employ a similar arrangement (diagram 2) merely substituting hinged shutters for the lamps, and turning them edgeways towards the observer when they were not required to be seen.

Lieutenant-Colonel the Hon. John Lindsay, in the year 1809, patented a system (diagram 3) which appeared to possess considerable merit. Lindsay's system for day signalling depended on the different positions of a triangular object suspended on a centre, and the combi-

nations of those positions with the positions of a circular body capable of being revolved around the triangular one. For night signalling this inventor made use of lanterns placed in various positions, as shown in the diagram. This system, owing to its comparative simplicity, contrasted very favourably with some of later date.

Rear-Admiral Henry Raper, in the year 1827, made a further step in advance, when, at that date, he patented "A new and improved system of signals; first, for communicating by day by means of flags and pendants, between ships at sea or other objects far distant from each other; in which system the colours of the flags and pendants, which have heretofore served to distinguish the signals from one another, and which by distance or other causes, are extremely subject to be mistaken, may be dispensed with altogether; and, secondly, for communicating by night between ships at sea, and other objects far distant from each other, by means of lights, and which system of signals is more conspicuous, expeditious, and certain than any which has hitherto been employed for the like purposes." (Illustrated by diagram 4.)

The principal peculiarities of this system were thus stated by the Patentee :—"They consist, first, in classing the signals according to their significations, and distinguishing the classes from each other, by combinations of flags and pendants; and, second, when colours are not visible, the employment of the distant signals by which they are made to express every signal, and even the telegraph. These signals are numeral, and for the first time, classed under distinct heads, such as movements of the fleet, signals *to* chasing ships, signals *by* chasing ships, engaging, compass signals, distress, ship's numbers, telegraph, &c.; and every signal is numbered in its respective class. Each class is distinguished by an appropriate combination of flags and pendants, by which means, the most conspicuous combinations are adapted to the signals of most importance, such as signals for engaging, chasing, distress; and the colours of the flags and pendants, which compose each combination, point out the number each signal bears in its particular class. Eleven flags and eleven pendants are required." "Experience," said the Patentee, "may improve these forms, but the colours are of little importance." He relied on the distinction between flags and pendants for determining

the general nature of his signals, and said accordingly that, whenever a flag could be distinguished from a pendant, a signal to a chasing ship could not be mistaken for a movement of the fleet ; nor could either of these be mistaken for a signal made by a chasing ship, or for the number of a ship on the list of the navy ; and the same reasoning applies to all the other combinations. By this it would be understood, that according to Rear-Admiral Raper's system, the character of the combination of flags and signals determined the class to which the signal belonged, and enabled this class to be identified without its being necessary to distinguish colour for that purpose. This appeared to have been an important step in the right direction, and to have greatly lessened the chances of error previously existing. It would, however, occupy too great an amount of the limited time at the author's disposal to enter into all the details of Admiral Raper's system, which, however imperfect it might be, as compared with existing systems, was evidently in advance of any system then in use.

It would appear, in so far as the author was able to form a judgment, that both for naval and military purposes any system of signalling employed should be capable of communicating with either one or many points or positions at once. Thus, that an admiral might either communicate with one ship only, or simultaneously with several or all the vessels under his orders. A general ought, in like manner, to be able to signalize any one or number of the regiments constituting his army. Hence, it appeared to be a matter of necessity that the signals made should present the same appearance all around. Farther, it must be borne in mind, that as these signals were *most* vital in their importance, at the very times when the minds of those who had to work and to interpret them were agitated either by the storm of battle or that of the elements, they ought to be of the simplest character, and as independent as possible of the skill and coolness of those who had to make and to receive them; and although for the purposes of engineering these requirements might, at first sight, seem less essential in their character, the author could not but think that numerous cases were likely to occur, in which the agitation consequent on imminent danger might, even in connection with engineering operations, be rather necessary to be taken into account.

He would take, for example, a little party of surveyors, laying out a line, and temporarily isolated from their companions. How uncommonly cool and collected these gentlemen would feel themselves, on discovering a party of painted and grinning savages in their immediate neighbourhood! He thought that under these circumstances a simple means of "telegraphic dispatch" would be wished for by most of them. But taking even a less romantic and more comfortable case, such as the floating of the great tubes of the Britannia and Conway Bridges, he apprehended that none of his hearers would deny the utility of a simple system of signalling, whereby the operation could be thoroughly and completely controlled without danger of misinterpretation; and he trusted, ere the conclusion of his paper, to bring to notice a system whereby this could be effected. It was unnecessary, however, to enlarge upon this point; as, if signalling was capable of useful application to the purposes of the engineering profession, he was confident of their being sufficiently progressive to avail themselves of it.

Systems of signalling had been divided into two classes; namely, the "General" system, wherein the symbols were displayed in groups, and the "Telegraphic," wherein the system of grouping was not used. Each of these had their acknowledged advantages and objections. The former was objectionable owing to the number of objects usually required to be displayed at one time, or, as it was technically termed, "at one hoist;" but was advantageous on account of the amount of meaning conveyed at one time. The telegraphic system was acknowledged to be good where only one position had to be addressed, inasmuch as it admitted in that case of very rapid change of objects; more than proportional to the greater amount of meaning conveyed at once by the general system.

Form, colour, sound, and motion had either separately, or in combination one with another, all been enlisted for signalling purposes.

The first systems to which he should refer, as belonging to the present or recent state of the subject, were those which were based upon the use of flags, variously grouped, and made use of to indicate the numbers affixed to the various significations of a pre-arranged code. Of such systems, it was obvious that those would be best which depended least upon colour, inasmuch as at certain distances,

and in certain states of the atmosphere, all colours resolved themselves into either dark or light, and it became impossible to farther distinguish their character. The ranges, too, at which the several colours might be seen varied considerably, so that if colour was an essential feature in a system, its effective range must be taken as that at which the least penetrative colour employed in it was visible. Beyond this range, that was, when a colour ceases to be distinguishable, there was obviously a liability that any two flags of similar form and pattern might be mistaken one for the other, and that in some cases serious consequences might result.

If these remarks were true of day signals, how much more so were they of night signals, as hitherto used? In considering this part of the subject, it must be borne in mind, that in using coloured flags for *day* signalling, the signal reached the observer by means of reflected light; but that in the case of night signalling, where lanterns were employed, a part of the light generated within the lanterns themselves had to be transmitted to the observer, and that every coloured glass interposed between the light and the observer greatly diminished the range. Of the colours employed, *red*, which absorbed the least amount of light, had the effect of diminishing the range one-third; *green*, being still worse; and *blue*, almost entirely obscuring a light. Then again, the difficulties of hoisting lanterns in various forms, and ensuring their being visible in their proper relative positions, on every side, on ship-board in a heavy sea, were such as to amount almost to impossibility. Under these circumstances, it was scarcely to be wondered at, that while day signals, as used in the Navy, numbered some fourteen thousand, it had only been considered that fifteen could with certainty be made at night. That such a vast disproportion existed between the requirements of a fleet in the day, and its necessities at night, it was obviously absurd to suppose; yet, such had been the proportion of communicating power hitherto relatively possessed during day and night. That such a state of things was lamentably bad, especially in the case of a nation having such vast naval interests at stake as our own, could not for a moment be denied; but to find a cure for the evil was apparently a matter of such difficulty as almost to give it the appearance of

impossibility. The author should remark that, *nominally*, the night system of the Navy was capable of making one hundred and three signals, but the number he had previously given was the limit of those which could be depended upon. The modes proposed for increasing the number of signals by means of what were called multipliers, consisting of mast-head-lights, rockets, blue-lights, guns, &c., were very objectionable, and had been condemned by our most experienced and best informed signal officers. In some cases, indeed, serious accidents had been caused by their failure to answer the end required.

Returning, however, to the subject of day signals. It had been justly observed that the most obvious and simple way of making a signal—say, of number—would be to display the number painted or written upon a card or other surface, or otherwise to display the corresponding number of flags or other objects; but the impracticability of such a system was too obvious to need demonstration, and it was hence necessary to make use of symbols which should act as a kind of short-hand, and enable us, with comparatively few objects variously placed or exposed, to represent a great range of numbers or other characters.

Now, in employing flags in which form, colour, number, and position constituted the elements of distinction of one signal from another, numerous systems had been suggested, and many had had their share of use; but those which had been, properly speaking, generally employed, and their use continued in its integrity for any length of time, were very few in number; if, indeed, any really original system could be said to have been employed since the earliest days of the art of signalling. The fact was, that in this, as in all other arts, improvement had gone on, not by great strides and sensation innovations, but progressively and by gradually striking out the bad and combining the good points of the various proposed systems.

Flags were usually employed with reference to a recognised code, which consisted of a collection of words or sentences, to each of which a number was affixed, which number was, therefore, symbolized whenever it was desired that the observer should act upon the word or sentence to which it was affixed, and which it consequently repre-

sented. This being understood, the author would proceed to call attention to one or two of the most prominent systems of flag signals.

Firstly, then, there were the "Numeral" and "Vocabulary" flags, made use of in the Royal Navy. In this system, five colours were employed, namely, white, black, red, yellow, and blue. Now, from what the author had previously said concerning colour, it would naturally occur to his hearers that such a variety was most objectionable, and that the liability to error in such a system must of necessity be very great. But it must be borne in mind, that in speaking of colours he had said that, under certain circumstances, all colours resolved themselves into *dark* and *light*, and that no other distinction between them could be depended upon. With this fact, then, before them, they would find on analysis that red, black, and blue, resolved themselves into dark, and yellow and white into light, colours. Now, on looking into the system, they would find that, for its purposes this distinction of dark and light colour was all sufficient, and that in the whole series of forty-seven flags and pendants, there were only two cases in which the dark and light colours assumed the same shape in different flags. From this it would be seen that, except in the cases referred to, colour might be entirely dispensed with, and the distinction of black and white only employed.

To show this more clearly he would take four of the numeral flags, represented in their proper colours, and the same four flags in black and white only. A comparison, would he thought, show the plain black and white to be the clearer of the two. The same fact might be observed with reference to the two cards exhibited, the uncoloured one being clearly visible at a greater distance than the one which was coloured.

With reference to this point of coloured and uncoloured flags, signal officers of experience stated that the latter were decidedly superior in distinctness to the former, and that where errors in reading occurred, the proportion was three, and even four, to one, in favour of the uncoloured.

America, a country from which, of all others, one would expect to derive improved ideas upon this important subject, had produced two sets of flags, which, like Hogarth's well known picture of per-

spective, were only likely to effect good by pointing out what ought to be avoided. These two strikingly bad systems were those of Mr. Rogers and Mr. Ward, and their faultiness was not only apparent to naval men of experience, but would, the author thought, at once be clear to the members. To illustrate this, he would take five flags of Rogers's system properly coloured, and a corresponding set of five flags in black and white only, and would ask whether they did not appear to have been specially designed with a view to enable the three upper and two lower flags to be mistaken one for another. These flags however, bad as they were, were greatly superior to Mr. Ward's, in which the principles to which the author had referred regarding dark and light colours seemed to have been completely ignored, and, as had been remarked by a naval officer conversant with the subject, "It really would seem as if, in devising this arrangement of flags, special preparation had been made for as large a number of errors as possible; as seven or eight flags done in black and white would represent everyone of the twenty-seven used in the set, and a simple black flag would stand for no less than eight of them." The consequences of carrying faults such as these into practice, were likely to be of the gravest character, and suggested that none but those who were practically acquainted with the requirements and difficulties of naval signalling were competent to design such systems of signals as could with safety be employed.

The author would not enlarge farther upon the subject of flag signals, which, however good and useful for the purposes to which they were applied, were scarcely likely to be largely available for engineering uses, inasmuch as the system was necessarily somewhat cumbrous, and required an amount of skill and practice for its efficient working far beyond what was ever likely to be devoted to it by engineers or their employés. But, before leaving the flag system, he would call attention to a method which had been suggested for displaying flags for army signalling, and seemed likely to be very useful. It consisted in sending up a small balloon, to which was attached a line of any required length, having upon it the necessary flags, which, in this manner, were enabled to be displayed from positions otherwise shut out from signalling.

Turning, then, to the subject of *night* signalling; it had been seen

that, whereas by day, our navy could make some 14,000 signals ; at night, it had to manage its business with fifteen; certainly a disproportionate number. The author did not know whether he was right or not in the supposition that much of our backwardness in the matter of night signalling had arisen from an idea that all improvements in it must show a continually nearer approach to the flag system, and must be founded on the same principles. At all events, this appeared to him to have been the principle followed out in the greater number of night signal systems, and to have acted as a drag upon the improvement of them; for, as he hoped to convince the meeting, there was, in reality, no difficulty in signalling by night to a greater distance than by day; nor in making any number of signals required in the most simple and effective manner. Perhaps, however, in saying, this, the author was somewhat anticipating his subject, and ought rather to explain the leading systems under which night signalling had been "*attempted*." He made use of this expression advisedly, for he considered that any system that could only produce fifteen effective night signals against 14,000 available in the day, could only be characterised as an attempt (and a very poor one too) at achieving the end desired. He would own that, when the subject was viewed under the fettered ideas consequent upon a desire to imitate the flag system, the difficulties to be encountered seemed very great, and it was only by acting upon an entirely different train of thought that anything like a simple and efficient system of night signalling could be arrived at ; and it required a spirit of considerable originality and boldness to strike into the new track.

Long before the date of the Christian era, two very curious systems of night signals were described by the Greek historian, Polybius, who, in the history of the "Punic Wars," makes frequent allusions to the art of signalling. These were as follows :—

The first, which has been aptly termed the "Pitcher" system, was thus arranged: "The two stations, between which signals were to be conveyed, were each supplied with a tall pitcher filled with water. A small tap was fitted to the bottom of each vessel, so that on opening it, the water might run out of both at exactly the same rate. A cork was set floating in each pitcher, and into each cork was fixed a long flat piece of wood divided into so many divisions of three fingers

breadth each as there was room for, and in each of these divisions a sentence likely to be used in time of war, was inscribed. Each station being provided in addition with a torch ready for lighting, was in a position to commence operations after dark.

"The station wishing to send one of the messages inscribed on the sticks, first showed a lighted torch, and kept it showing until answered by another. Then the first station opened the tap of its vessel and dropped the torch, which was the signal for the other station to do the same. Both corks and sticks were now descending at the same rate, so that as soon as the sentence required to be transmitted reached the level of the top of the vessel at the *first* station, the tap was stopped and the torch again shown, so that the *second* might do the same and read off the message."

The second system described by the same writer was thus carried on:

"Each station wishing to signal prepared five pieces of wood on which the letters of the alphabet were written in their proper order, five on each slip, except the last, which contained four. The person wishing to signal, first showed two torches as a 'preparative,' and then the message was *spelt* as follows: Torches from one up to five displayed on the left, signified the number of the tablet, and then so many torches on the right the number of the letter on that tablet. The letter 'delta' would thus be signified by one torch to the left and four torches to the right, and so on, throughout the course of the signal."

Considering the date at which they were produced, the systems just described were highly meritorious. They had been pronounced capable of working fully as effectively as many of far more recent date, and were believed to have been competent to convey messages to distances of from four to five miles in a clear atmosphere, and to have been distinctly visible at such distances to the naked eye. Hence it had been pertinently asked, with reference to the comparative difficulties of day and night signalling, "By what possible arrangement could signals be made visible at that distance in the daytime?"

They now came to consider what had been done in the way of night signalling in their own day, always keeping in view what the author had stated with reference to the effect of coloured glasses in diminishing range, and consequently limiting in like proportion the effec-

tiveness of any system in which colour was employed. It was unnecessary for their purpose to enter into a detailed description of all the various systems of night signals hitherto in ordinary use, as their character would be sufficiently understood from the diagrams shown. Suffice it to say that they almost all required the use of several lamps, which in some cases had to be shown in the forms of squares, triangles, &c. In others, the lamps, which were coloured, were suspended vertically over each other, the order of the several coloured and uncoloured lamps being varied to convey different meanings. Others, again, made use simply of uncoloured lanterns, which, for the reasons the author had explained, were superior, where the simplicity of the systems as regards working was equal, to those in which colour was employed. With this before them, it might probably appear to some of the hearers, while comfortably seated in a warm room, that night signalling by these means was not such a very troublesome matter after all; that lamps might be strung up, either in the form of triangles "or any other form," and changed about from one order to another with the greatest facility; but, unfortunately, the conditions under which signalling could be effected in *that room*, and those under which the operation had to be carried on *at sea*, were materially different. In "night and storm and darkness," the experience of signal officers was, that to hoist a string or frame of lanterns at all, without some of them being blown out or otherwise rendered ineffective, was no easy matter, and that to do it in such manner as to display them effectively all around, was frequently all but impossible. In the case of lights required to be seen all round in the forms of triangles or squares, it was of course necessary that the frame carrying the lights should be kept continually revolving, as, otherwise, some of the observers would see the lights in line with each other, and the character of the signal, in so far as they were concerned, would be entirely changed.

The "semaphore," which, as the hearers were aware, was formerly our principal means of inland signalling, consisted simply of a tall post, to which were jointed two arms, capable of revolving in vertical planes, and of being set to any required angle with the post, or with each other. These arms were actuated by chains, and gave 27 changes in all to the instrument. It was obvious that, by making

use of his arms to represent those of the instrument, a man might, on this principle, communicate messages to a considerable distance and they accordingly found that several systems of the kind had been used.

One or two other systems of like character to the semaphore had been invented, but did not appear to have been much employed.

They now came to another class of day signals, namely, that in which solid forms were made use of, and which, for many purposes, seemed decidedly preferable to the class in which flags were employed. A gentleman in the East India Company's service, seemed to have been the first to propose the use of solid forms for signalling, and was followed by Captain Grant, who, in 1838, patented a system comprising nearly all the forms which could be practically used. Among the solid-form systems, also, was Lieutenant Colomb's Multiform Telegraph, an apparatus which displayed much ingenuity, and of which a model was before them. The whole apparatus was extremely light and portable, and was capable of more than 12,000 distinct changes, visible from every point of view. Lieutenant Colomb said it was principally with a view to get rid of the difficulties attendant upon the display of solid shapes in the ordinary manner, that he invented this apparatus.

The next invention that had to be considered, was known as "Redl's Cone Telegraph," regarding which, the author quoted the following remarks by a thoroughly competent authority: "We now come for the first time upon a system, certainly having its own apparatus as a foundation, but being perfectly applicable besides, as far as a telegraphic system can be, to any objects under any circumstances, without requiring any additional study after the system is once learnt." This, in itself, was high praise, and it could not be disputed that the system in question was well deserving of it. The apparatus, as originally constructed, consisted of four hollow cones united in pairs at their bases. They were formed, internally, of hoops, which served to keep their outer covering expanded as necessary, but which could be drawn up or down, one within the other, when required; and the apparatus, as then constructed, required to be hoisted from a gaff or yard, as the case might be. For the adaptation of this system to military purposes, it was proposed by Lieu-

tenant Colomb to make use of cones, constructed somewhat in the manner of umbrellas, closing round a pole as in the model there exhibited. This arrangement was at once simple, portable, and effective in its character, and for movable stations, was, doubtless, preferable to any that could be devised. The system of numbering the cones was as follows: supposing them to be all open, the top represented 1; the next lower, 2; the next, 3; and the lowest, 4; 5 was then formed by the first and fourth; 6 by the second and fourth; 7 by the third and fourth; 8 by the first, third, and fourth; 9 by the second, third, and fourth; 0 was formed by the second and third cones, as there was then some resemblance to the Roman numeral X. There were then other changes, as shown in the diagram. In working short distances with this system, it had only been found necessary for the station signalled to repeat the "stops," instead of repeating the signal throughout. By adopting this plan, it appeared that the speed of transmission had reached at Aldershot, 24 figures per minute. The "alphabetical" arrangement was similarly simple in character, and was as follows: Whenever it was necessary to signal by letters, the number 5 was taken as representing *a*, and the rest of the letters were numbered onward, according to their order. The reason for the *a* being numbered 5 instead of 1, as would seem most natural, was, that if it were so, the figures 13 would equally represent *a*, *c*, and *m*, whereby confusion would arise; but by making the 5 represent *a*, the figure 1 could never represent a letter by itself, but must necessarily belong to the figure which followed it.

With regard to the universal application of this system, the author quoted the following lucid remarks from a paper recently read before the Royal United Service Institution, on the subject of signalling: "There are two states in which each cone can rest; closed and open. If, then, we say that one thing, no matter what, represents an open cone, and another thing represents a closed cone, if we run through the series of four changes, we can express the fifteen variations of which the apparatus is capable. Suppose, for example, the *right* arm represents an *open* cone, and the *left* a *closed* one; if, now, we raise the *right* arm *once*, the *left twice*, and, lastly, the *right* once, we shall have expressed—*first* cone *open; second, shut; third, shut; fourth, open*, or the figure 5. So with any other two things; as a black flag and

a white ball or flag,—a hat and a stick,—a short sound and a long one,—a single and a double sound, or a low and a high note,—any two things, in fact, no matter what or where they are." This system had, to some extent, been adopted in the Army, but good as it undoubtedly was, would, the author thought, give place to others of still more effective and perfect character. Yet, even granting that this be the case, it was evident that at the time of its introduction, it constituted a very great step in advance, and more or less pointed the way to something still simpler, and capable of yet more extensive application.

The system to which the author had now to call their attention, was that known as the "Flashing System" of Lieutenant Colomb, R.N., which, as carried out by him, was not only applicable to, and entirely overcame the difficulties attendant upon night signalling, which by it was enabled to be carried on with even greater ease, and to a greater distance than signalling by day; but was also reduced to so simple a matter in the transmission and reading of signals, as to come within the range of the most ordinary capacity, and to be learnt by any practised signalman in the course of an hour's, or, by others, in the course of a day's time. According to Lieutenant Colomb's system—which was the result of long experience in the requirements of the subject, combined with an extensive series of carefully conducted experiments, carried on under the circumstances of actual practice—*one uncoloured light* only was employed. This light was contained within a lantern, glazed in such manner, as that the light, when uncovered, might be visible from any point of view, and it was on the exposure and obscuration of it that the system was founded. Thus, *one short* flash or exposure of the light signified 1.

Two short flashes, 2.
Three short flashes, 3.
Four short flashes, 4.
Five short flashes, 5.
One long flash, 6.
One short, followed by *one long* flash, 7.
One long, followed by *one short* flash, 8.
Two short flashes, followed by *one long* one, 9; and,
One long, followed by *two short* flashes, 0.

The other necessary signals of the code were made in a similar manner, by short and long flashes variously combined, as shown in the diagram.

The mode in which the signals were produced was singularly ingenious and effective. In the interior of the lamp (of which diagram No. 6, was a magnified vertical section) was a cylindrical shade of peculiar construction, which fell by its own weight, and completely covered the light; but when raised, as completely exposed it. This shade was connected by means of a cord, with a lever in the signal-box (of which diagram 7 was a magnified plan with the cover removed, and diagram 8, a corresponding end view), which contained an apparatus closely resembling a barrel organ. The surface of the barrel (which was capable of being steadily revolved by means of a small winch-handle) was occupied by four series of pins and bars, each series representing the numeral flashes as previously given. These pins and bars were so arranged, that after the first series had passed any point, a slight interval elapsed before the next commenced, and so on. Between the end of the fourth series and commencement of the first, there was an interval amounting to one fourth the circumference of the barrel; the object of these intervals being to separate the figures, one from another, and as every signal was continually repeated till answered, the long interval was necessary to distinguish the beginning and end of the series transmitted. Parallel to the barrel, was a square iron bar, upon which were fitted five short levers or keys, capable of being moved endwise into various positions along it. Above the bar was fixed a flat brass plate, termed the "director," having engraved upon it four series of figures from 1 to 0, together with the titles of the several naval signal books. Opposite to each figure and title, and in the line of revolution of the corresponding set of projections on the barrel, was a notch into which a small catch, one of which was pointed to each of the levers or keys, could be moved, thus bringing the corresponding lever into a position to be acted upon by the projections. On now turning the handle, the revolution of the barrel caused the pins upon its surface to operate upon the short levers or keys, which transmitted their motion to the quare bar, upon the end of which was fixed a lever, from which

motion was communicated, through another lever and a cord, to the lamp shade.

By what the author had said it would be understood that, with this apparatus, it was only necessary to place the catches in the notches corresponding to any required series of figures, and then to turn the handle continuously, to produce a revolving series of flashes, corresponding to the setting of the instrument, with the most unerring accuracy.

It was evident that, as every signal might thus be repeated three or four times per minute continuously, none of the accidental interruptions to signalling at sea, such as the intervention of passing vessels, ropes, smoke, or sails, could render the progress of night signals, under this system, more uncertain than flag signalling was by day, when subjected to the same interruptions. The large apparatus then before them had been for some time in use in the Channel Fleet, on board of Her Majesty's ship *Edgar*, from which it had been kindly lent to the author for the purposes of his paper. Its success in the Fleet had been most unequivocal,—a fact not to be wondered at, when the extreme simplicity of the principle, and the evident certainty of its action, were considered. When the apparatus was required to be made use of for day signalling, the cord, instead of being attached to the shade of a lamp as described, might be attached to, and made to open a collapsible cone or other opaque object, during intervals corresponding with the short and long flashes or exposures of the light in the night system. (See diagram 9.)

The author much regretted that the time at his disposal would not permit of his entering into particulars of several systems, well deserving of notice. Among these, he might mention the very ingenious lime light arrangements of Captain Bolton, already partly adopted in the Army, and several others of considerable value. But to enter fully into all the details and arrangements bearing upon the question before them, would occupy the time of many meetings, and, notwithstanding the great general importance and interest of the subject, would be apt, he feared, to become wearisome. As it was, he had endeavoured as far as possible to give them a sketch of the main features of the question; and as regards night signalling, had, he

thought, brought before them a system, and an apparatus, which fulfilled their requirements in a remarkably simple and perfect manner. That under the very imperfect arrangements in ordinary use, collisions and other disastrous accidents had not been more frequent than they confessedly had, was certainly to be wondered at; but as it was many had been acknowledged to have occurred entirely in consequence of this imperfection, and he could not doubt that, in thus producing a means whereby night signalling became so easy and certain in its character, Lieutenant Colomb would, in the end, obtain the gratitude of all naval men, and find his system adopted in all the navies of the world, whether war or mercantile.

In conclusion, the author would express his thanks to Lieutenant Colomb, Mr. Wm. Nunn, the well known signal lamp manufacturer, and several other gentlemen, for the kind assistance they had afforded him.

DISCUSSION.

Captain Gilmore Harvey stated that it would take an hour to signal a message by the old plan, which might by the new plan be done in a few minutes. By the old form of squares, the proper form could only be accurately observed by vessels abreast of each other. By Mr. Colomb's apparatus, this could easily be obviated. This system was considered by himself and his brother officers to be a very great advantage over any form of signal previously used.

Mr. Le Feuvre said, as the author of the paper had referred to the system of signalling adopted some time before the Christian era, he (Mr. Le Feuvre) might perhaps mention, or rather refer to, the system of signalling hitherto adopted by the Arabs in the northern parts of Africa, namely, a living semaphore. Men were employed, and by the position in which they held their arms, danger or safety was signified. It was a system that was very effectually carried out, stations being placed all over the country. Indeed, the Arabs carried out this system so effectually, that it became a source of great annoyance to the French Government, for the fact of a number of men advancing, could be signalled for a distance of fifteen miles. Since then, however, this system had been superseded by the electric telegraph, which traversed the desert. He (Mr. Le Feuvre) thought sig-

nalling might be carried out successfully in large works. For instance, in the International Exhibition, while the material was being hoisted up, the "navvies" had flags, the colours of the flags being red, green, and blue. He differed with the author of the paper as to the system he had brought before the meeting as far as regards the day signalling, for he should in all cases adhere to colour; but, as regarded night signalling, he thought it would be a great improvement upon the plan now in general use. The only difficulty was that of being able to catch the exact numbers when at a distance; unless the numbers were noted very minutely, one might be missed now and then, especially in foggy weather. The general system, he thought, was somewhat complicated, and he was of opinion that some method might be adopted by which less mechanical labour was employed; for, be it remembered, it was not a question of a large number of signs, as only ten were required. While upon this subject, he might, perhaps, refer to a subject which had received but little attention, namely, Fog Signals. He thought means might be devised to improve the present system; at any rate, he hoped the question would be touched upon during the discussion, as it was one of the greatest importance, but at the same time of difficulty.

Mr. H. P. Stephenson considered that the system under discussion was a great improvement upon that formerly in use, though he thought a difficulty would be experienced in distinguishing between a short and a long flash, particularly when the sea was running high.

Captain G. Harvey said, that as every ship answered the same signals, much greater regularity could be maintained by machinery than by manual manipulation. Each signal was repeated until it was correctly returned.

Mr. H. P. Stephenson thought the object could be better obtained by colour.

Mr. Nunn said, that by using colour signals, the range would be shortened; for example: a bright light could be seen three miles, a red light only two miles, and a green light one mile. The chief object was to do away with colours, to secure signals being made at long distances.

Mr. Roberts could not see what practical difference there could be between a man pulling a string and the machine used; the levers

of the machine had to be shifted and adjusted for the required signal, and a man who learned to work the machine, could learn to regulate the flashes by pulling the string.

Mr. Nunn said a man would not do the work so well as the instrument, and thought it was going backwards to substitute manual for mechanical labour.

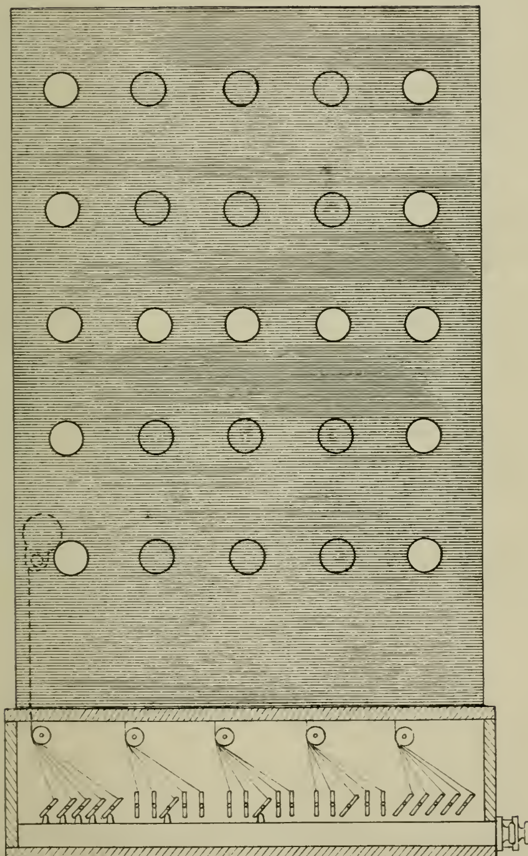
Mr. Olrick was surprised to hear engineers advocating manual instead of mechanical arrangements. As regarded the system of signalling under discussion and those formerly in use, he considered there was no comparison, and any one who had seen the two systems in practical use at sea, would be of the same opinion. The signals under the old systems were not understood by half of the ships; he had seen several systems applied, but they were all defective, whereas the light signal of Mr. Colomb was beautifully accurate. A mistake in counting the flashes, or confusing one signal with another, could not be made, as each signal was repeated until it was answered by a corresponding signal from the ship signalled.

Mr. F. Young quite agreed with the opinion expressed by Mr. Olrick, that it was not for them to advocate hand labour against machinery; and he was sure, after what they had seen that evening, they could not help feeling satisfied that the system introduced was perfect. Colour, instead of form, was every way objectionable. When crossing the Atlantic, during the laying of the electric cable, the colour of the signal flags of the ships speaking could not be detected. Of course, if there were a plane surface shown, it could be made out, but seeing it on edge, as it were, it could not be distinguished. The mistaking one signal for another was obviated by repeating it until acknowledged. Some short time since, when travelling along a straight piece of railway, he particularly noticed the fact adverted to by Mr. Nunn, namely, that coloured lights were distinguishable at different ranges, for while the white light could be seen for a mile, the green and red were closely approached before they were seen. All he could say relative to the system of signalling introduced to the meeting was, that while he thought some improvement could be effected in the manipulation, the principle deserved every commendation.

Mr. A. F. Wilson thought the light from the oil lamp was too

BOAZ'S NIGHT SIGNALS.

A.D. 1801.



E. AND F. N. SPON, BUCKLERSBURY, LONDON

Upfield Green. Litho

Diagram 2.

BOAZ'S DAY SIGNALS.

A.D. 1801.

E. AND F. N. SPON, BUCKLERSBURY, LONDON.

Upfield Green, L.

LINDSAY'S DAY SIGNALS.

A.D. 1809.

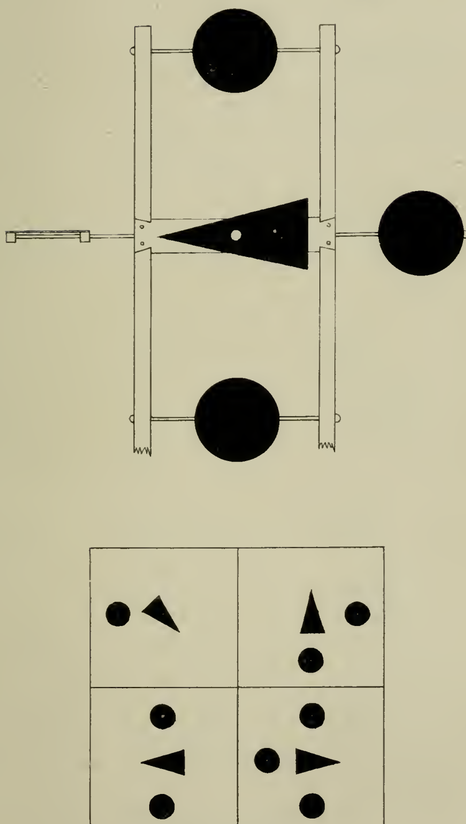


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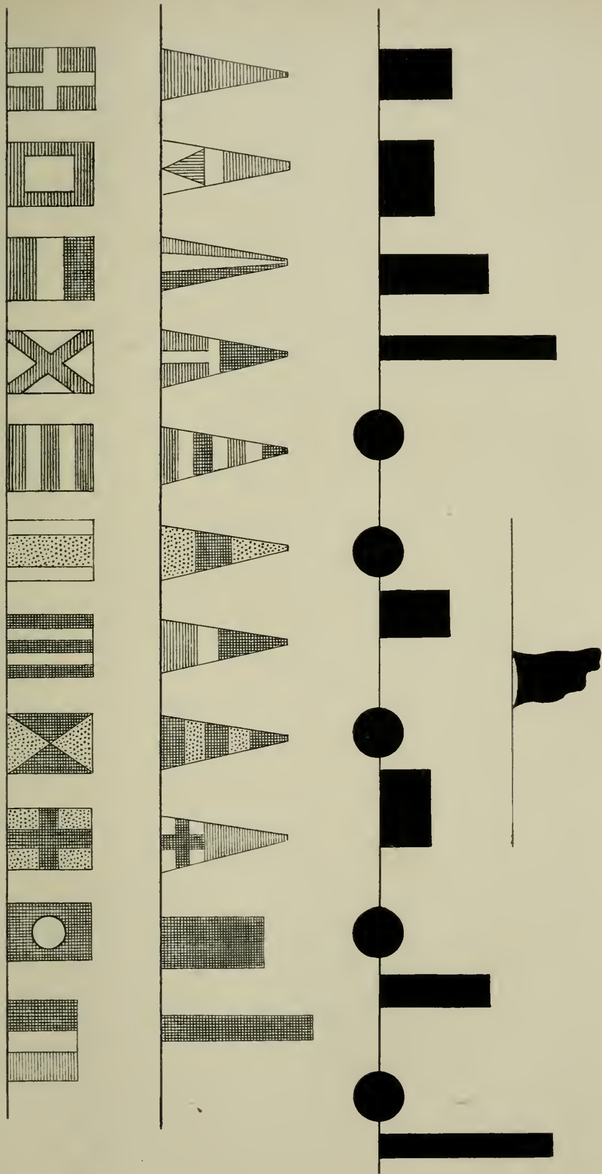
Diagram 4.

RAPER'S SIGNALS.



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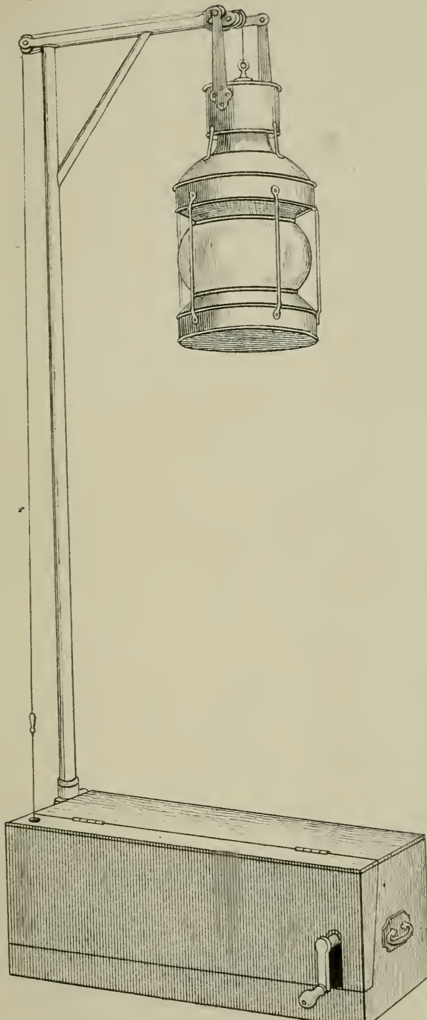
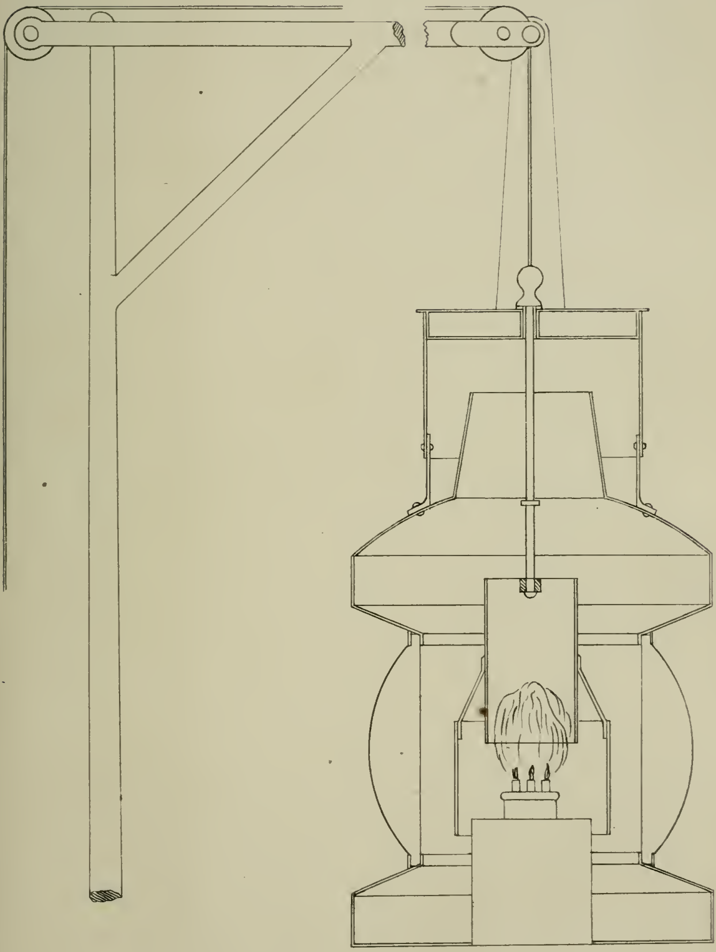
COLOMB'S FLASHING SIGNALS.



APPARATUS COMPLETE.

COLOMB'S FLASHING SIGNALS.



VERTICAL SECTION OF LAMP.

F. N. SPON, BUCKLERSBURY, LONDON

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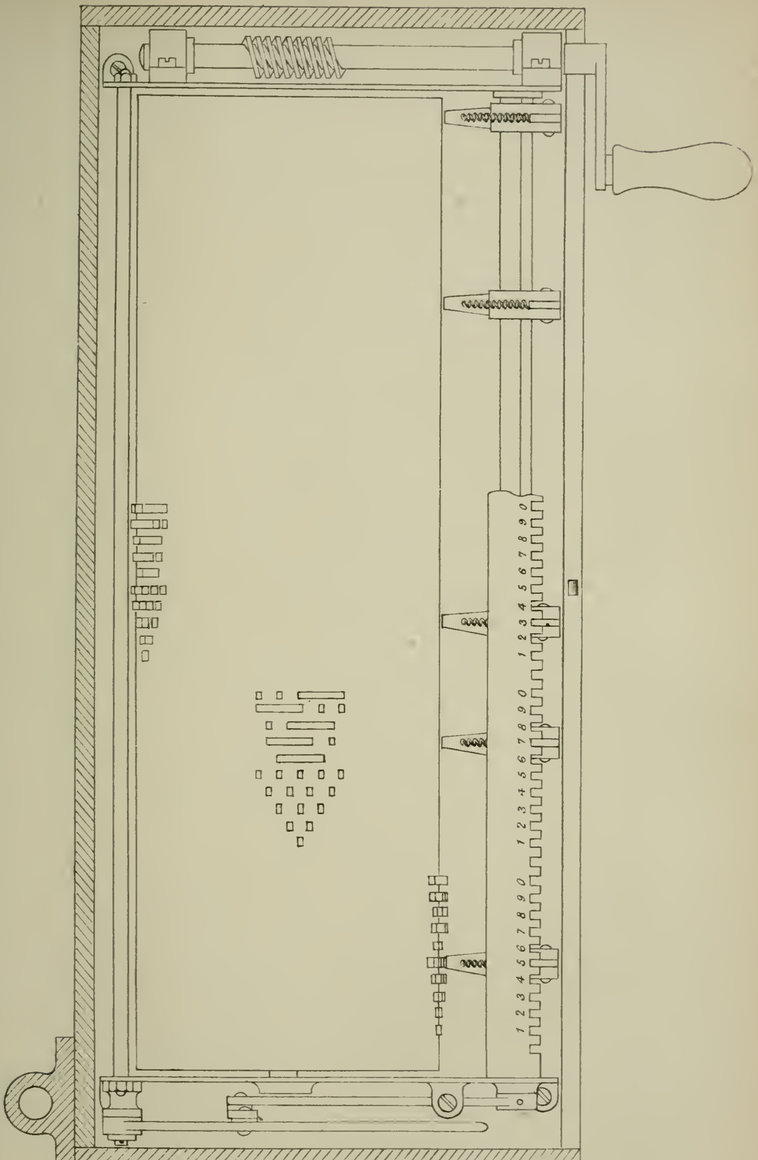
COLOMB'S FLASHING SIGNALS.

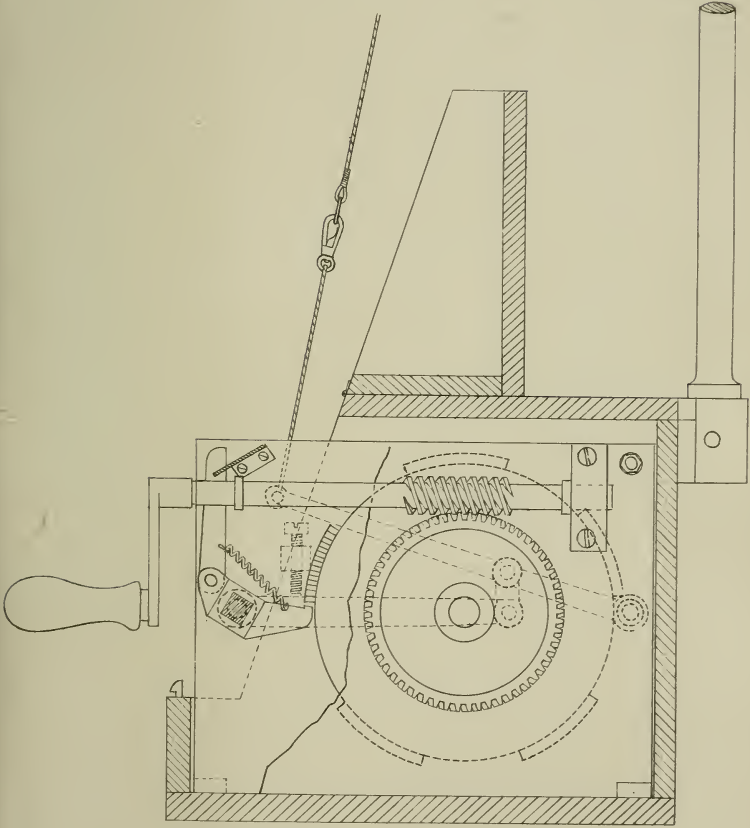


PLAN OF SIGNAL BOX WITH COVER REMOVED.

E.N. SPON, BUCKLERSBURY, LONDON.

Upfield Heen Lith.

COLOMB'S FLASHING SIGNALS.

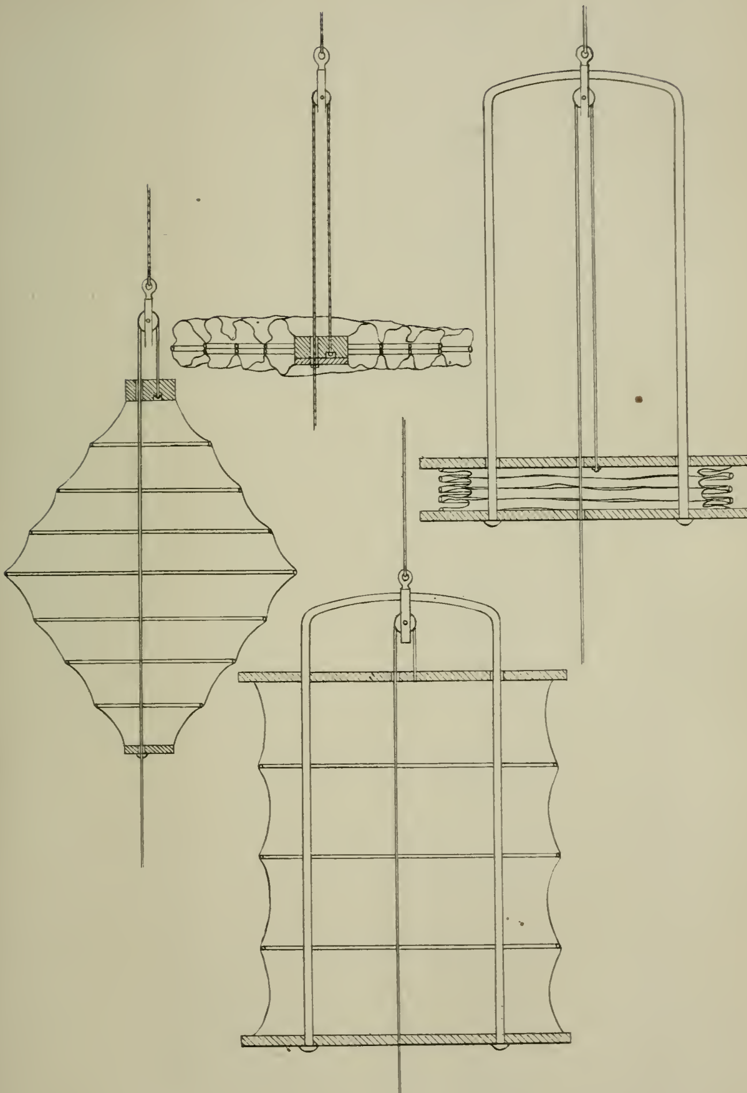


SECTION OF SIGNAL BOX.

E AND F. N. SPON, BUCKLERSBURY LONDON

Upfield Green, Litho

COLOMB'S DAY SIGNALS.



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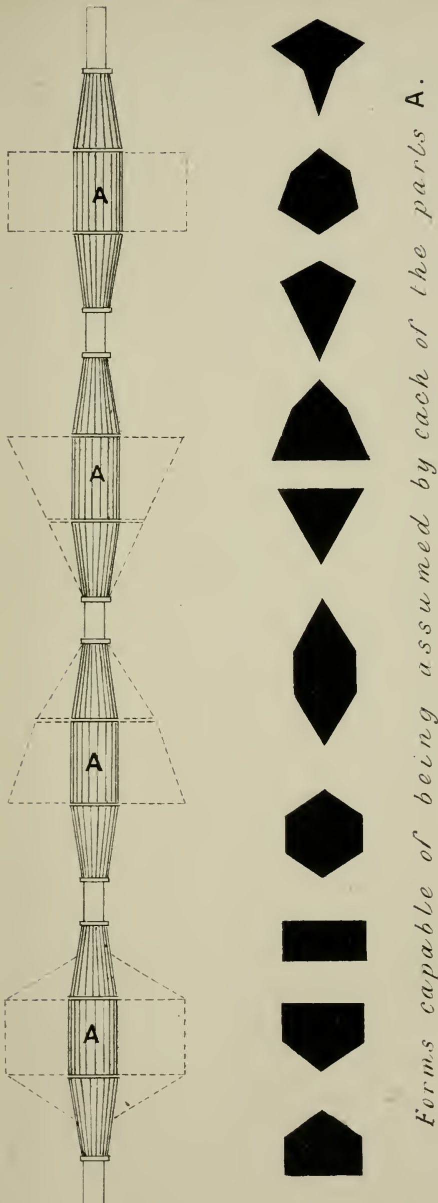
COLOMB'S MULTIFORM TELEGRAPH.



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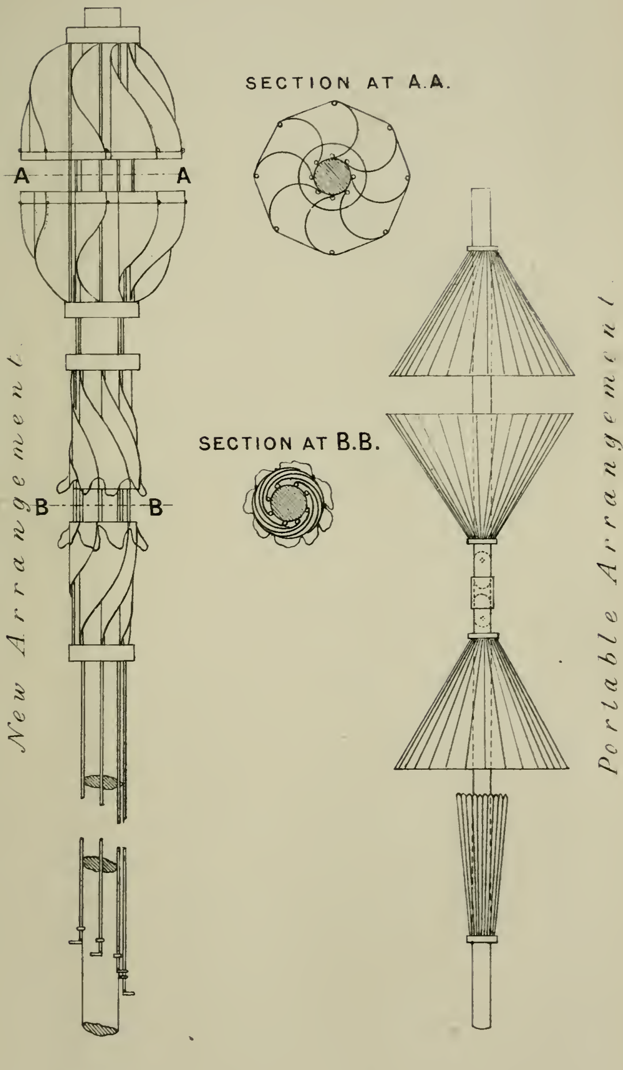
REDL'S CONE TELEGRAPH.



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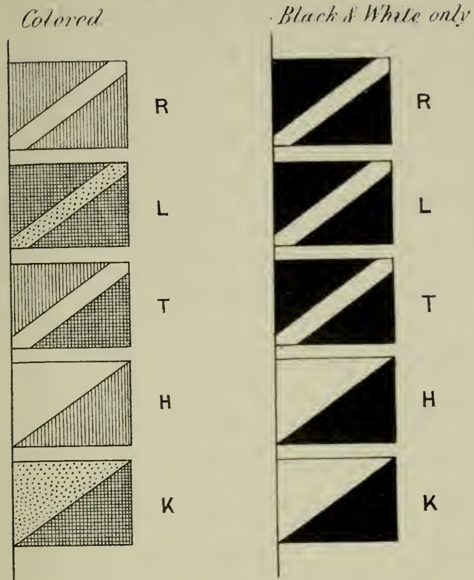
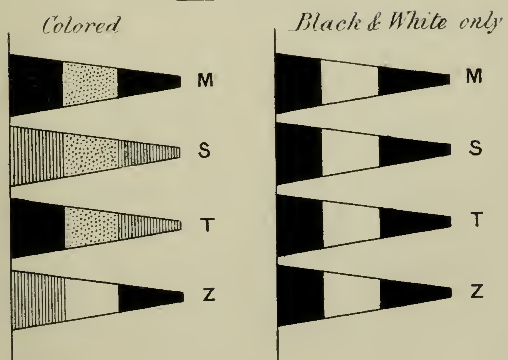
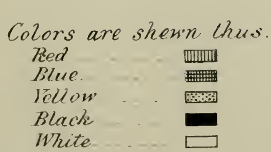
ROGERS' FLAGS.



WARD'S FLAGS





AND F. N. SPON BUCKLERSBURY, LONDON

Upfield Green, Litho

dim, and suggested that the same system should be carried out with an electric light.

Mr. Nunn did not approve of the electric light, which had hitherto proved a failure, especially on the night of the late illuminations in honour of the Prince of Wales's wedding. The light exhibited could be seen at sea at a distance of $6\frac{1}{2}$ miles.

The Chairman said that he had had no practical experience of the working of these lights. The only objection to the system of signaling proposed, but which was obviated by repeating the signals, was the liability of missing the short flashes, or as they came rather rapidly, to mistake the end of one figure for the beginning of another. As regarded the use of a lime light, he thought it was difficult to use it at sea. A mast-head lantern might be suspended, as that gave a very powerful light which could be seen at a considerable distance, and as no reflectors were used the light could be seen in every direction. The umbrella signals appeared to be useful, but he did not know that they were better than signalling by arms, and, moreover, he thought there would be considerable difficulty in using them in a high wind.

December 7th, 1863.

R. M. CHRISTIE in the Chair.

STEAM NAVIGATION ON THE INDUS.

By Alfred Warren, Assoc. I.N.A. (late Marine Engineer to the Indus Steam Flotilla).

The subject of this paper is one that can hardly fail to be of interest to engineers. The Indus is at present the only artery of commerce between the North West of India and the Western coast, and it must continue to be so until the completion of a railway up the valley of the Indus—an event which can hardly take place for ten or twelve years at least. It is, therefore, of great importance that an efficient steam communication should be established on the river. The attempts hitherto made to accomplish this have, however, met with so little success, that it becomes interesting to inquire into the

cause of the failures, and to ascertain, if possible, the best way of remedying them in future.

Description of the Indus—Its Course.

The Indus rises in Thibet, but little is known of it until it issues from the Himalayas, at Attok, where it is joined by the river of Cabul. Small steamers have ascended the river as far as Attok, where there are rapids which it is not possible to ascend. At Mittenkote, at the south-western extremity of the Punjaub, the Indus is joined by the Chenaub or united rivers of the Punjaub, all of which flow from the Himalayas, and are for the most part navigable by small steamers. After this the Indus receives no farther affluents during the whole of its course of about 500 miles to the sea. The portion of the river on which regular steam communication is now established is between Kotree, opposite Hydrabad, in Scinde, the upper terminus of the Scinde Railway, and Moultan on the Chenaub, the lower terminus of the Punjaub Railway. The distance, measured along the banks, is 570 miles, but the navigable channel is much longer than this. The Delta lies below Kotree, and is seldom now navigated by steamers.

Rise and Fall of Water.

The water of the river is subject to a rise and fall of 17 ft. to 19 ft. every year. During the winter months, or from the end of November to the middle or end of March, the water is at the lowest—in some places not exceeding 2 ft. or 3 ft. in depth. It then rises gradually till the end of August, remains a few days at the highest, and then falls gradually till the end of November. The rise of the water is due almost entirely to the melting of the snows in the Himalayas, and is hardly affected at all by the rainfall, which is very small in that part of India, and generally occurs only on a few days in July and August.

Current.

During the low season the current in the Indus is three to four miles per hour, and in the Chenaub two or three miles. When the water is high the current in the Indus varies in different parts from four to six miles an hour, and is, occasionally, even more than this; and in the Chenaub it is three to four miles.

Channel.

The width of the river between the permanent banks varies very much. In some places it is not more than half a mile wide; while, in others, it spreads out to a width of some miles. In the wider places islands are formed, which become covered with low brushwood, but are liable, from year to year, to be washed away and formed again in other places. The water of the river contains so much sand in suspension, the current is so strong, and the eddies so numerous, that the formation and washing away of sand-banks is a process continually going on, and the navigable channel is, in consequence, continually shifting. At the height of the inundation the permanent banks are always a good deal washed away. Villages may then be seen undermined by the water, and falling into the river. During the inundation of last year (1862) the flourishing town of Mittenkote, the largest and most important place between Sukkur and Moúltan, was entirely washed away.

It has happened that a steamer has made fast at night under the shelter of a projecting portion of the bank, and in a few hours the bank has been washed away, and the steamer cast adrift. A steamer was nearly lost in this manner in August, 1861. The banks are generally low, flat, and uninteresting except at Sukkur and Sehwan.

Sukkur Pass.

At Sukkur there is a narrow pass between high rocks about half a mile in length. There is a bend in the river just above the pass, and two islands in the pass. The current has been gauged as high as 9·9 knots, or more than $11\frac{1}{2}$ miles an hour. The eddies are very violent, and at such times there is much difficulty and danger in getting up the pass. This prodigious current, however, will only last for a few days at a time during the rise of the waters. During the low season the current is not stronger there than elsewhere.

Navigable Channel during the Low Season.

The navigable channel during the low season is narrow and shallow. It winds about from side to side of the bed of the river, and not only changes its position after every inundation, but is continually chang-

ing it, so that a channel which is navigable one week will often not be so the next, another channel having been formed. The bottom, in many places, rises as the water rises. In 1861, when the water was high, there was a place below Kotree where there was said not to be more than five feet depth of water all across the river. Many of the bends of the river are exceedingly sharp, and in such places the current is almost always very strong. The most difficult period of the year for the navigation is during the fall of the river. The channel that has been formed during the low season is generally silted up or absorbed into a wider channel as the water rises. When the water falls again, another channel has to be formed, and during this process it is sometimes difficult to find the channel at all, and the changes in it are very rapid.

Native Pilots.

With such varying channels it would hardly be possible to navigate the river at all were it not for the skill of the native pilots in what they call "reading" the water, *i.e.*, telling by the appearance of the surface what the depth is. They are very rarely mistaken, but they sometimes take a channel which has become stopped up, and then they have to turn back and seek another channel. Englishmen who have commanded steamers on the river for years say that they could never learn to read the waters as the native pilots do.

Wooding Stations.

In such channels it is impossible for steamers to proceed in the dark, and they are obliged to make fast to the bank. Wooding stations occur at every twenty or thirty miles, and steamers always endeavour to reach a station before nightfall, so as not to lose time by wooding during the day, wood being hitherto the only fuel used.

Government Steamers.

The first steamers were placed on the Indus during the conquest of Scinde, and were of material assistance to Sir C. Napier in his military operations. After the conquest other steamers were built, with an especial view to the peculiarities of the navigation. They were principally employed in the conveyance of troops and stores,

which were carried in an accommodation flat towed alongside. Passengers were conveyed by them, but general merchandise was not sought for.

Dimensions.

The four largest of these steamers, built by Messrs. Laird, of Birkenhead, are in—

	ft.	in.
Length on water-line	168	0
Length on deck	175	0
Beam	28	0
Draught when quite light	3	3

Steeple engines by Messrs. Maudslay and Field, 110-horse power:—

	ft.	in.
Diameter of cylinder	0	43
Length of stroke	4	0
Diameter of paddle-wheels	17	6
Width of do.	8	6
Length of engine-room	35	0

Speed.

Their ordinary working speed, without a barge in tow, is twelve to thirteen miles an hour.

Form.

They are spoon-shaped stem and stern, but they have a movable dead wood, which can be raised up and down, and is generally left down as the rudder is then found to act more efficiently. The spoon form of these vessels has been strongly advocated for two reasons. First, on account of the supposed facility for getting them off sand-banks; and secondly, because they are thought to be less affected by the eddies of the river. The author was of opinion however, that these supposed advantages were to a great extent fallacious. A spoon bow will take the ground nearer amidships than an ordinary bow, or, if the vessel is going with any speed at the time of striking, it will be liable to lift itself on the sand-bank, and then it would be more difficult to get it off than if the bow had been wedged in. With

respect to the effect of eddies on the bow, there may, perhaps, be some advantage, but he could not observe any superiority in their steering qualities when in troubled water.

Construction.

These steamers are very strongly constructed. There are five transverse water-tight bulkheads and two longitudinal bulkheads which rise about 2 ft. above deck at the engine-room, and give great longitudinal stiffness to the vessel.

Draught and Capacity.

The draught, with all stores and fuel on board, is 3 ft. 9 in. or more. They are therefore incapable, when the water is low, of carrying cargo on board, but they can then tow a barge carrying 100 or 120 tons, or two barges with the same quantity, but it has been customary to put the cargo into one barge only. When the water is high and the current strong they may carry 80 to 100 tons on board, but are not then capable of towing a barge.

Defects and Merits.

The chief defects of these steamers are their small carrying and towing power, and their great draught. On the other hand, their speed is good, and the experience of several years shows that they are capable of withstanding the wear and tear of the river service better, probably, than any other steamer on the river

Oriental Inland Steam Navigation Company.

The first company started for placing steamers on the river was the Oriental Inland Steam Navigation Company. Their plan was to carry out Mr. Bourne's patent train of barges. The steamer was 200 ft. long, 20 ft. beam, and 5 ft. deep. It had a spoon bow and convex stern. The first or connecting barge was 40 ft. long, and 18 ft. beam, with concave bow and stern. There were to be four other barges 100 ft. long, 18 ft. beam, and 5 ft. deep, with convex bows and concave sterns, so as to fit close into one another. The hindermost barge had a spoon-shaped stern. It was supposed that by this plan a number of barges could be towed with the bow resistance of only the one steamer. The result of a trial in the harbour

of Kurrachee in 1859 is singular.* The speed of the steamer alone was 9·37 statute miles, with 273 indicated horse power, a very small speed if the steamer was in good working condition. With the 40 ft. barge, and one 100 ft. barge, loaded to 2 ft. 10 in., in tow, the speed was reduced to 7·19 miles with 303 indicated horse-power. With a second 100 ft. barge, the speed was reduced to 5·75 miles with 332 indicated horse-power; and with a third barge to 4·33 miles, showing a diminution of speed for the first barge of 2·18 miles, for the second of 1·44 miles, and for the third of 1·42 miles. With such a speed it was of course hopeless to effect anything on the Indus, but an attempt was made to ascend the river with one barge only in tow beside the connecting barge; it proved, however, an utter failure. The train was found to be quite unmanageable in the sharp bends and cross currents of the river, and after some further trials the train system was abandoned. The steamer and barges were altered so as to act in the ordinary way, two barges being towed, one on each side of the steamer; her speed has always been slow, but she has run pretty regularly with her two barges until the spring of this year, when she unfortunately grounded amidships and broke her back.

Steamers from the Clyde.

The company, finding that the trains were a failure, determined to purchase some of the Clyde steamers, and send them out to India. One, the *Windsor Castle*, was taken to pieces in the Clyde. The hull was shipped in a vessel which went to the bottom before it had got to sea. It was therefore determined to build a new hull for the engines. This hull was made 30 ft. longer than the old one, but the same in other respects, and an additional boiler was provided for the engine. This steamer was launched at Kurrachee in the autumn of 1861, and called the *Indus*. Her length over all is 218 ft. 7 in.; beam, 19 ft. 6 in.; depth, 7 ft. 5 in.; draught with all stores and fuel on board, full 4 ft.; engines, 120 nominal horse-power. She has a very fine entrance and run, and her speed at the trial in the harbour was said to be above 21 miles an hour. She has towed up the river three barges carrying 1500 hogsheads of ale, or about 430 tons. This,

* From Mr. Leys' paper on "Indian River Steamers," read to the Scottish Shipbuilders' Association.

however, was under favourable circumstances of the river, and she could not do that on an average. Probably two barges with 150 tons in each would be a fair average load for her. Owing to her great draught she is incapable of carrying any cargo on board.

Merits and Defects.

The great merit of this steamer is its large towing power, but it may be doubted whether the same power might not be obtained in a steamer better suited to the navigation of the river. A steamer built for a speed of twenty miles an hour is surely not the best adapted, and, therefore, not the most economical for towing barges at the rate of twelve miles. If a vessel is not intended to go faster than twelve miles an hour, there can be no advantage in giving her a form suited to twenty miles. It was, he believed, an acknow-edged principle, that if a vessel were fine enough to pass through the water at a given speed without raising a wave, there would be no diminution of resistance at that speed by giving her a finer form, but, on the contrary, by increasing the length, the surface is increased, and thereby the frictional resistance. He was of opinion, therefore, that a steamer of shorter length, but greater beam and less draught, and the same engine power, would have quite as much towing capacity and be more manageable and useful in the river; for it is certain that the shorter the length and the less the draught, the more suitable is the steamer to the navigation of the river. The chief defect of this steamer is its excessive draught.

Harbour Tug.

This company have also a harbour tug steamer at Kurrachee. Others of the Clyde steamers purchased by them were lost in their passage out to India. Another of the train steamers altered is now in course of erection.

Indus Steam Flotilla Company.

The next attempt to establish steamers on the river was made by the Scinde Railway Company. Their object was to have steamers running in connexion with the Scinde and Punjaub Railways, so as to have a continuous steam communication from Kurrachee to Lahore.

A company was, therefore, formed in 1858, called the Indus Steam Flotilla Company, in order to carry out this object. Designs and tenders for steamers were obtained from various shipbuilders. The principal conditions laid down were, that the length was not to exceed 200 ft., nor the draught of water 2 ft., and the power to be sufficient to tow a barge with troops or cargo. The tender selected was by Mr. J. Scott Russell, and as there were some peculiarities in the design and method of construction, it was determined to have one steamer erected and tried on the Thames before ordering more. The dimensions of this steamer, the *Stanley*, are as follows :

Dimensions.

Length, 200 ft. ; beam, 38 ft. ; depth on side, 6 ft., with houses on deck ; engines, 120 nominal horse-power ; diameter of cylinder (three in number) 34 in., oscillating, working on same crank pin ; stroke 4 ft. ; ordinary marine boilers, 25 lb. pressure ; paddle-wheels, 14 ft. 6 in. diameter. In form she was flat-bottomed, with long straight sides. The bow is 60 ft. long, the stern 40 ft., and the centre or straight part 100 ft., or half the whole length. She has hollow water-lines in the bow, and the bottom rising at the stern to the height of 2 ft., with convex water-lines. In the steamers subsequently built the bottom rose to a height of 3 ft. 6 in. She was tried in the Thames in February, 1859, and was said to have a speed of thirteen miles an hour. The draught was under 2 ft., and the indicated horse-power was 663. This was considered such a favourable result that it was at once determined to have six other similar steamers built. The contract, however, was not given to Mr. Russell, but tenders were again invited. The specification was for hulls similar to that of the *Stanley*, with a few alterations, intended to give greater stiffness and strength, but the design of the engines was left to the contractor, the horse-power only being specified—120-horse power nominal. The contract was given to Messrs. Richardson and Duck, of Stockton, the engines being made by Messrs. Kitson and Hewitson, of Leeds.

Longitudinal System.

The method of construction of these vessels is somewhat peculiar. They are not constructed in the ordinary way, with transverse ribs 2 ft. to 3 ft. apart, but are on what Mr. Scott Russell calls the longi-

tudinal system. He considers that in the ordinary method the large quantity of iron put into the ribs of a ship does not impart strength where it is required, and is, therefore, to a great extent wasted. He says, to quote his own words, "In the longitudinal system I take all that excessive, and, as I think, comparatively useless quantity of iron, which is usually formed into ribs, and placed across the ship, loading it with a weight which contributes far less than its due proportion to the general strength, and I place it lengthways along the skin so as to contribute materially to the strength and efficiency of the skin, where it is much wanted, and to give strength to the structure as a whole, where it is at present weakest. To give the best idea of the difference between the new system and the old system, I give the following example of ships of 600-700 tons scale, built on both systems.

WEIGHTS OF IRON USED IN THE GENERAL STRUCTURE.

	Old System. Tons.	New System. Tons.
In the skin	110	110
In transverse internal strengthening	130	40
In longitudinal ,,	40	130

It thus appears that the iron expended in strengthening the ship longitudinally in the new system is to that in the old in the proportion of 130 to 40.* It would certainly appear that this system has great advantages for light flat-bottomed vessels, of shallow draught and liable to ground, where longitudinal stiffness is the great desideratum, and comparatively little is obtained from the skin of the vessel. The author then explained the construction of the steamers built by Messrs. Richardson and Duck.

DETAILS OF CONSTRUCTION.

There are two longitudinal bulkheads 22 ft. apart, running the whole length of the ship. For 142 ft. amidships, *i.e.*, to the end of the deck houses, of which they form the sides, they have a height of 14 ft. 6 in. Beyond this they are carried below deck tapering at the ends to about 3 ft. 6 in. There are seven keelsons on the bottom,

* Transactions of the Inst. of Nav. Arch., vol. iii. p. 164.

and two on the sides, three of them 18 in. deep, and the rest 9 in. In the engine-room they are all 15 in. deep. There are five transverse water-tight bulkheads. Those at the ends of the engine-room are 14 ft. 6 in. in height between the longitudinal bulkheads. Fore and aft the engine-room there are ten of what Mr. Russell calls partial bulkheads, or bulkheads with the centre part cut out, forming a continuous girder round the vessel, 18 in. deep at the bottom and 15 in. at the sides, the deck beam being 5 in. deep. These are 11 ft. to 12 ft. apart, and there are no ribs between them. In the engine-room they are about 6 ft. apart, and 15 in. deep. Stringer plates, 18 in. wide, run round the gunwale and along the top of the longitudinal bulkheads. The plates of the skin are $\frac{3}{16}$ in. thick under the engine-room, and 8 wire gauge elsewhere. The plates of the bulkhead are 8 and 9 wire gauge. The iron is charcoal iron, manufactured at the Pontypool Ironworks, and is of excellent quality. The decks and wood fittings generally are of teak. No other wood is capable of standing the heat of the climate. The engines extend over the whole length of the engine-room, which is 52 ft. long, and rest on box girders, 2 ft. deep, 8 in. wide, and $\frac{5}{16}$ in. thick. They are direct-acting, working at an inclination on opposite sides of the shaft, the cranks being connected by a drag link. The diameter of the cylinder is 34 in.; stroke, 7 ft.; diameter of wheels, 23 ft. There are four boilers, of locomotive shape, capable of working to 40 lb. pressure.

Objections to Longitudinal System.

It will be seen by the above description, and a reference to the drawing, that between the keelsons and bulkheads there are large spaces where the plate has no support. Some of these spaces are as much as 11 ft. 6 in. by 4 ft. 6 in. In a sea-going ship with thick plates, there is no objection to this; but when the vessel is constantly grounding, and the plates are very thin, the effect of the grounding is to bulge the plates inwards between the frames. This effect was just apparent in the *Stanley* after one voyage up the river; and it is to be feared that, in a year or two, it will become serious. It may be doubted whether this defect will not more than counterbalance the advantages of the longitudinal system.

Performance of the "Stanley" in India.

The *Stanley* was sent out to India and re-erected there, but she never had the speed said to be attained in the Thames. Her engines were raised 18 in., and the wheels increased 3 ft. in diameter. This alteration improved the working of the engines, but the speed hardly exceeded eleven miles. She made a trial trip up the river to Moultan in December, 1860. The water was unusually low at the time, and her light draught enabled her to pass over shallows where Government vessels were aground; but her great size made it very difficult to steer her in narrow channels and shallow water. All steamers steer more or less badly in shallow water, and the greater the extent of flat bottom the worse they steer. This effect is particularly seen in passing from deep water into shallow water. Steamers on approaching shallow water will often fly off against their helm, while there is still several feet of water below the bottom. This defect was very manifest in the *Stanley*, and he believed it was impossible to remedy it. In deep water she steered with great ease. The paddle-wheels caused great commotion in the water, raising large waves at the stern. This was no doubt due to the great length of straight sides, the extent of flat bottom and want of a fine run, which prevented the water escaping freely from the wheels. The *Stanley* is now running regularly on the river.

Performance of the other Steamers in India.

The first steamer built by Messrs. Richardson and Duck was tried in Kurrachee harbour in April, 1861. The engines were found defective in two particulars. The condensers could not maintain a vacuum, and the boilers primed so much that it was found impossible to prevent the water from passing over into the cylinders. These defects were remedied after a delay of some months, after which further delays took place, and it was not till the spring of this year that the steamer was sent up the river. None of the other steamers have been tried. This result is very much to be regretted, for he was persuaded, though these steamers were not all that might be desired, yet that they are capable of doing good service in the river, and that there is no sufficient reason why they should not all have been at work on the river by this time.

Great Capacity.

These steamers have one great merit which has not yet been tested They have great capacity for carrying cargo at a light draught. They are capable of carrying 250 tons, including fuel, or, say, 220 tons, exclusive of fuel, at a draught of 3 ft. 6 in.; he believed it would be far preferable to put all this cargo on board than to tow it, or a portion of it, in a barge alongside, as has hitherto been done.

Deck-House.

The house on deck is certainly a mistake. The object was to provide a cool cabin for passengers, but in the extreme heat of Upper Scinde this result is not obtained; on the contrary, the heat in the cabin of the *Stanley* was once at 112 deg., with an awning over it. In cabins below deck the temperature would never rise much above 90 deg., and with a punkah this heat is quite bearable, and indeed feels cool on passing into it out of a temperature of above 100, which the air generally has during the day in summer. The deck-house greatly increases the difficulty of managing the vessel, and opposes considerable resistance to the wind.

Corrosion.

The author here mentioned a singular circumstance which had occurred to one of these steamers. She was launched at Kurrachee in the beginning of 1861, and remained at anchor in the harbour. She was perfectly tight till this year, when she was suddenly found to be leaking in several places through the centre of the plates of the bottom. She was put on shore at once, and then it was found that her bottom was full of holes, having the appearance of being eaten by worms. He never before heard of an instance of worms eating through iron. This company have lately purchased six of the best of the Government steamers with the dockyard and machinery, and the Government establishment is broken up. They have also some tugs and a number of barges of galvanised corrugated iron, but they have been found too small, and altogether unsuited to the service of the river.

New Government Steamer.

A steamer and barge for transporting troops, of very large dimensions, have lately been built by the Government, according to the recommendations of a commission appointed by them in 1857, to report on the navigation of the shallow rivers of Europe. The steamer is 350 ft. long, 45 ft. beam, and 2 ft. draught, spoon-shaped fore and aft, 120 horse-power. (He believed these were the correct dimensions.) She is strengthened longitudinally by two arched girders. The barge is 220 ft. long. The speed of the steamer at the trial trip at Kurrachee is said to have been 8 miles an hour. She has got up to Kotree, and the Government has placed her in the hands of the Indus Steam Flotilla Company, in the hopes that they may be able to work her, but there is little hope of her being of any use in the river. She is much too large to get up the river when the water is low, and she has not nearly power and speed enough to stem the current when the water is high.

Drawing No. 6, shows plans of all the different steamers on one sheet. The differences between them may be seen at a glance.

Having thus described the peculiar characteristics of the river, and pointed out the merits and defects of the various steamers at work upon it, we have sufficient data to lay down the principles which should guide us in designing steamers for the river.

Length.

Steamers should not exceed 200 ft. in length. This may appear an unnecessarily small limitation of length, as the most powerful steamer on the river is 210 ft., but he had pointed out that this steamer would probably do her duty better if she were somewhat shorter. It is certain that small steamers, 150 ft. long, have always made their journeys more regularly than the larger ones in the low seasons, and it must be remembered that this season extends over four months, and is the busiest season of the year. The *Stanley*, which is 200 ft. long, has been aground fore and aft across the main channel of the river in by no means the narrowest place. If she had been much longer than she is, there would have been great difficulty in getting her up the river at all at her trial trip, when she was

without a cargo. The universal opinion of commanders of steamers on the river is, that 200 ft. is the extreme length that a steamer should have, and that it is preferable to keep within it.

Draught.

The extreme load draught should not exceed 3 ft. 6 in., and it would be better to keep it to 3 ft. Steamers drawing 4 ft. are sometimes delayed for weeks in the passage up and down the river when the water is low, while those drawing under 3 ft. are seldom delayed seriously.

Carrying and Towing.

They should be easily manageable in the shallow, narrow channels, sharp bends, and strong currents of the river. This opens the question whether towing or carrying steamers are preferable. Hitherto towing has been the universal practice, but he thought there was some ground to question its correctness. It is certain that, in such a river as the Indus, a steamer by itself is more manageable and less liable to accidents, and will make her journeys with much greater regularity, than one having barges in tow. On the other hand, she could hardly carry as much cargo. The example of the steamer *Indus* shows that we may rely on a tug steamer to tow two barges, with 300 tons, up the river. A steamer of suitable size for the river could hardly carry more than 200 tons at 3 ft. 6 in. draught, but this is more than any of the steamers formerly belonging to the Government are able to convey. This question can only be determined by practical experience, but the author is strongly of opinion that proper carrying steamers will be found the most efficient.

Form.

The form should be suited to a speed of twelve or thirteen miles an hour. The spoon bow is not a necessity. The hollow bow of the *Stanley* answers perfectly well. At the same time the performance of the spoon bows in shallow and in troubled water is good if it is not rough. A spoon stern is objectionable, as it affects injuriously the passage of water to the rudder. Straight sides should be avoided, and it would be better to give the bottom a rise of an inch or two, rather than keep it perfectly flat.

Construction.

The construction of the steamers should be such as to enable them to withstand constant grounding without injury by straining or in other ways, and should be as light as possible.

Engines.

With respect to the engines nothing has hitherto been said, but there is no great difficulty about them. Oscillating engines have the advantage of having no exposed rubbing surfaces, which is desirable in the case of sand storms, which frequently occur, at times lasting for hours, and doing much injury to exposed surfaces. The steeple engines of Messrs. Maudslay and Field have, however, worked well on the river for years. The direct acting engines of Messrs. Kitson and Hewitson have the advantage of spreading the weight and pressure over a great length of the vessel, but they are not compact, and the arrangement of the boilers is open to objection. Surface condensers should certainly be provided in consequence of the intense muddiness of the water. The starting gear should be on deck. The paddle-wheels are very liable to injury. It has been found best to provide against this, not by making them very strong, but by making them very slight, so as to be easily repaired. If a wheel is seriously injured by striking a bank, as often happens, the steamer is made fast, the injured part is taken out and replaced by spare material, which is always kept on board, and in half an hour or so the wheel is entirely repaired, and the vessel proceeds on her way. Boilers must be provided with large furnaces for burning wood. They should have large water and steam room, so as not to be liable to sudden fluctuations of water level and pressure. As all the stokers are natives, this is a desirable precaution. The square marine boiler, with returning tubes, seems to have an advantage over the locomotive form of boiler.

Proposed Cargo Steamer.

The author then proceeded to give a rough outline of two designs for steamers, in which he endeavoured to embody the above principles, and which he ventured to think would be found efficient steamers for

the river. One is intended for carrying, and the other for towing. He first described the cargo steamer. Its dimensions being:

	ft.	in.
Length on 3 ft. water-line	190	0
Ditto, over all	200	0
Beam	38	0
Depth from underside of deck to skin . . .	9	6
Draught, with all stores, but no fuel, under .	2	0

1000 maunds, or 36·5 tons of wood fuel, equivalent to about twenty hours' consumption; 3 in. draught; 200 tons of cargo, 17 in.; extreme load draught, without fuel, 3 ft. 5 in.; ditto, with fuel, 3 ft. 8 in.

Form.

There is a slight rise of 3 in. in the floor, so as to avoid a perfectly flat bottom. The sides begin to taper almost at once from the midship section, so that there is no length of flat side. The water-lines are slightly hollowed, so as to give as fine an entrance and run as possible. There is a slight rise forward. The stern projects aft over the rudder. There is an advantage in this, as it protects the rudder, and enables the anchor-boat to be towed astern without fear of damage from the rudder, and it renders it possible to steer from the fly, which was found a great advantage in the *Stanley*. (The *Stanley* had a projecting stern added after she was built.)

Construction.

The method of construction is substantially the same as that of the Government steamers. The skin is supported by transverse ribs 2 ft. apart at the engine-room, and 2 ft. 6 in. elsewhere. The floors are 6 in. deep, and $\frac{3}{16}$ in. thick. Reverse angle irons are carried along the top of the floors, and up the frames at the sides. There are five transverse water-tight bulkheads. The longitudinal strength is obtained by two longitudinal bulkheads, fastened to the top of the floors and the deck beams, 22 ft. apart, and by four keelsons 15 in. deep at the engine-room, and 12 in. fore and aft. There will be deck beams 5 in. × 3 in. on each alternate frame, and a gunwale stringer 15 in. wide running all round the vessel. The engine bearers and

beams would of course have to be adapted to the engines. They should be made as stiff as possible, so as to avoid any straining of the engines when the vessel grounds. The plates should be of the best quality of iron; they would be $\frac{1}{4}$ in. thick in the bottom, and $\frac{3}{16}$ in. in the sides, and 9 wire gauge in the bulkheads. The angle iron generally would be $2\frac{1}{2}$ in. × $2\frac{1}{2}$ in. × $\frac{1}{4}$ in.

Engines.

The engines would be 110 horse power nominal; oscillating cylinders 43 in. diameter, 4 ft. stroke; diameter of paddle-wheels 18 ft., length 9 ft. To have surface condensers. Boilers to be of the form of ordinary marine boilers, to work to 25 lb. per inch, with large furnaces for burning wood. They should be supplied with superheating apparatus, and any other improvement which would tend to economise fuel, the great item of expense.

Passenger Accommodation

Could be provided in the after hold similar to that provided in the Government steamers; but it would, of course, materially diminish the space for cargo, and there is, in the steamers now at work on the river, quite sufficient present accommodation for passengers. Cabins would, of course, be provided for officers and crew.

Proposed Tug Steamer.

The dimensions of the tug steamer would be—

	ft.	in.
Length on 3 ft. water-line	190	0
„ over all	198	0
Beam	28	0
Depth from underside of deck to skin	9	6
Draught with stores, but no fuel	2	8

1000 maunds of wood fuel, equivalent to 4 in. draught. The form of this steamer is similar to that of the cargo steamer. The principal difference is a diminution of 10 ft. in the beam. The method of construction is precisely similar, with such adaptation as the diminished beam requires. The engines to be of 120-horse power nominal. They have more work to do, and should, therefore, have more power than

those of the cargo boat. As the hold is not wanted for cargo, the engines and boilers may be spread over a greater length of the vessel, by placing the boilers fore and aft of the engines. There is, however, an advantage in having the boilers altogether abaft the engines, as it keeps the engine-room and stoke-hole so much cooler. The barges for this steamer should be capable of carrying 150 tons at 3 ft. draught.

	ft.	in.
Length	150	0
Beam	22	0
Depth from underside of deck to skin . . .	7	0

The author thought it unnecessary to enter further into the details of construction of these steamers and engines, being anxious only to point out the kind of steamers which he considered most suitable for the navigation of the river, and the principles which should guide us in their construction; and he was confident that steamers of this description would prove altogether efficient on the Indus, and accomplish all that could be required of them. He then made a few observations with respect to the commercial view of the case. It has been stated lately, by the chairman of the Indus Steam Flotilla Company, that there is a much greater demand for freight than they are able to provide for, and that more steamers are urgently required. They have, indeed, obtained tenders for two tug steamers 275 ft. long to tow each two barges 200 ft. long. If this design is carried out, there can be little doubt that it will add another to the failures already made in navigating the Indus, as steamers of such a length will be useless for several months of the year, as has been pointed out. He hoped wiser counsels would prevail with respect to them. It is difficult to estimate, with anything like accuracy, the cost of such steamers as those described. The cargo steamer, when completed with all her equipment, would hardly cost less than 20,000*l*. The tug steamer would cost somewhat less, but the two barges would probably come to 4000*l*. apiece. No estimate that the author could make of the probable earnings would be of any value, but the steamer *Indus* earned on one occasion more than 2000*l*. on the freight of the down journey alone, and other steamers have earned as much for the double journey, and as the cost of a double journey

would not be more than 500*l*. exclusive of wear and tear, there is surely sufficient margin for profits.

It is owing to the misfortunes that have befallen both companies having steamers on the Indus, and the great consequent expenditure of unprofitable capital, that they have been unable, as yet, to pay any dividends. If suitable steamers could be placed on the river at a reasonable cost, and judiciously worked, there can be little doubt that they would earn very good profits. It must be conceded, however, that steam communication on the Indus can never be rapid. The up journey will always take on an average a fortnight or more, and the down journey about a week. When a railway is completed from Kotree to Moultan, the journey may be accomplished in a day. The actual cost of freight per mile is more by the steamers than by the Scinde Railway. There is, therefore, no chance of steamers competing with a railway for through traffic; but there will always be a considerable amount of local traffic on the river, which a railway could not accommodate, and for which steamers will still be required.

Drawing No. 7, shows the lines of a native Indian coasting trader. All the native-built vessels, whether coasters or small fishing-boats, are very fast sailers. The diagram at the top of the drawing may explain why they should be so. The lower line is the 3 ft. water line, supposing the vessel to be trimmed 6 in. by the stern. The upper line is one of Mr. Scott Russell's true wave lines. The coincidence between the two is remarkable, and if the wave theory is true, these vessels ought to be fast, as in fact they are. The rig is one large lateen sail with the mast raking forwards. The largest size vessels only have two masts. With this rig they cannot tack, but are obliged to wear when they change their course. This makes them unhandy for beating up against the wind, but when running before the wind very few vessels can beat them.

In conclusion, he expressed a hope that steam navigation on the Indus would be more successful in the future than it has been in the past. There are certainly great difficulties in the way; but difficulties are not impossibilities, and should only stimulate us to greater exertions, and encourage us to look forward to more gratifying successes.

DISCUSSION.

Mr. Bartholomew could not understand the object of putting the barge by the side of the steamer. If the channels were devious and narrow, it would appear that the steamer should cut the way for the barge to follow. By placing the barge at the side of the steamer, a wider channel of a certain depth was required. He did not exactly remember the draught of the barge, but he supposed it would be about the same as the steamer. As regards the holes which were found in the iron, that was a very important point, and one which required further investigation. He would ask Mr. Warren, if the question had been gone into, to see if there were any cause to account for it in the iron itself. He (Mr. Bartholomew) inquired if the same thing were observed in any of the other steamers, because, if not, it might arise from some peculiarity in the iron.

Mr. Olrick inquired whether any leakage occurred in consequence of the plates being damaged, and also whether the twin screw propellers had ever been tried upon the Indus.

Mr. Lidbetter (of Kurrachee) replied, that, with respect to the holes in the iron plates of the bottom of the *Sir Bartle Frere* steamer, the vessel was lying close to the beach at Keamaree Island in Kurrachee harbour. The water of Kurrachee harbour is supersalt, it receives no fresh-water streams into it except at rare intervals, perhaps once a year, when the Lyaree for a few hours rolls down a little water into the head of the harbour. The vessel was lying a few feet from the shore, just in sufficient depth of water to keep her from grounding at low water. Barnacles and seaweed accumulated very fast on iron vessels' bottoms in Kurrachee harbour, occasionally to the extent of 3 or 4 inches in thickness, and this was the case with the steamer in question. Suddenly this vessel sprang a leak, and on examination on the inside of the ship a number of minute holes were visible. On laying the steamer aground and scraping off the barnacles, it was evident that these holes were made by the Pholas, a seaworm very abundant in the harbour; the holes were clear of the rivets, some right in the centre of the plates. If he could obtain a piece of a plate with holes in, he would have pleasure in forwarding the same to the society. It seemed to him not to be the result of an

acid applied by the Pholas, but had every appearance of being done by mechanical action, and in his opinion was a case of real boring. The holes were $\frac{1}{4}$ to $\frac{3}{8}$ of an inch in diameter outside, but when dis covered were only the size of a pin's point on the inside. The plates n these steamers are under $\frac{1}{4}$ in. in thickness, and the holes had the exact and regular appearance of auger holes in wood.

Mr. E. Riley said there were many insects—some of the Mollusca, for instance—which possessed the power of attaching themselves to the bottoms of ships, &c., and of giving out an acid matter which corroded the iron. The results produced in the plates referred to could not arise from a mechanical action, it must have been caused by an acid.

Mr. H. P. Stephenson understood that one of the navigation companies of India had constructed vessels which drew only 2 ft. of water. He should be glad if Mr. Warren could state the performance of these boats. He thought the non-success, commercially, of some of these companies arose from the unfortunate way in which they had been managed. He had heard that one of them had made profits to the extent of 60 per cent.

Mr. Lidbetter said the profits of 60 per cent. mentioned by the last speaker were realised entirely upon the Ganges, and not on the Indus. In his opinion, the train system of Mr. Bourne—of a steamer, a connecting barge, and cargo, and passenger barges, to the length in all of about 600 ft.—was a total failure on the Indus. The steamer had a convex stern, and the connecting barge a concave and almost hemispherical bow, which held water and would always impede progression, and was fatal to good steering, and to the possibility of making such a connexion as to ensure safety with facility of working. The stern of the connecting barge was also concave, and the other barges had all convex bows and concave sterns, the bow of one being fitted in and jointed, in a manner, to the preceding vessel. Eventually the *Sutledge* steamer was altered, and adapted to tow the ordinary shaped barges alongside. This steamer was the only one belonging to the Oriental Inland Steam Company on the Indus for many months. There was no want of cargo to carry up and down the river; what was required to ensure success and make the company a paying one, was a sufficiency of vessels of the right

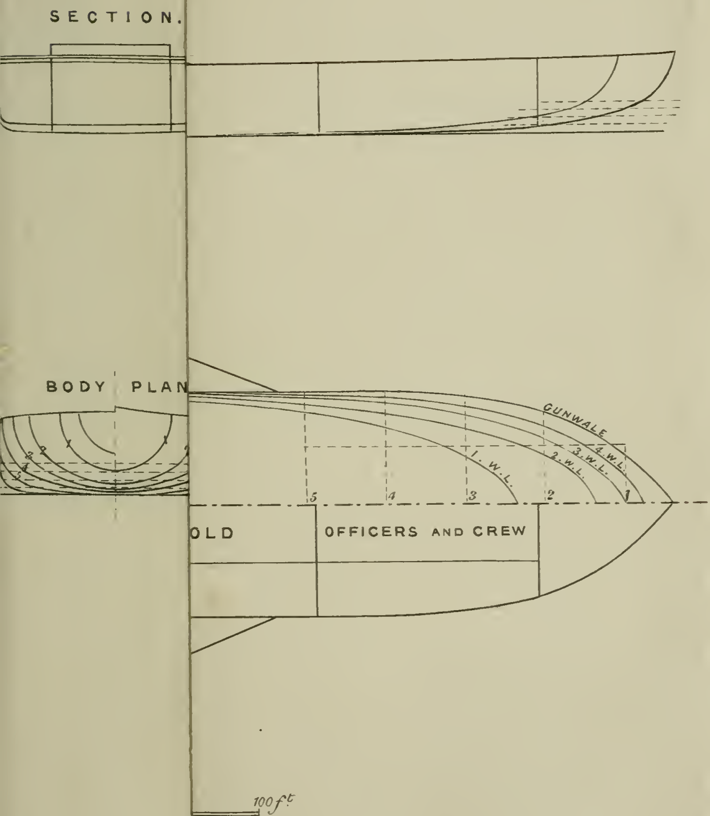
kind. The *Indus*, the second vessel of that name belonging to the same Company, had been very successful, she had towed down the river from Moultan to Kotree four barges, two on each side, laden with cotton, and other produce, and on some of these trips she must have grossed nearly 3000*l.* sterling, freight on the downward trip. Vessels of the class of the *Indus*, answer well on the river, as a whole, but they might be improved with respect to draught of water. Three feet should be an outside draught for vessels and barges on the Indus during the low season, but during the inundation, there was no limit to draught speaking comparatively. As even if the railway from Kotree to Moultan were sanctioned, it would take several years to complete, there was in Mr. Lidbetter's opinion, a great and open field for mercantile enterprise on the river Indus. Successful as the steamer *Indus*, before mentioned, had been, it was quite possible for any company with sufficient capital and well conducted, to build, and maintain on the river, a still more suitable and consequently successful class of steamers, which in his opinion would realise large profits to the company, in carrying merchandise, railway, commissariat and Government stores up the Indus to the Punjaub, and bringing down produce. In the present juncture especially, of warlike operations on our trans-Indus frontiers, steam tonnage would be largely in demand; and lamentable to say, from one cause or another, there is not now, and has never been since the trade was opened, a sufficient amount of available steam tonnage on the river, such as to give shippers of goods upwards or downwards the facilities of speedy and punctual delivery, much more to respond to any such emergency as the present. Mr. Lidbetter said that he had lately been on the Mississippi, and he observed that in all the steamers there the engines were unconnected, one engine to each wheel, and he thought this plan would be a great advantage on the Indus.

Mr. Warren said the reason the barges were attached to the side of the steamer was that in the sudden bends of the river with the current rushing round them, if the barge were astern it would be driven against the bank and it could not be steered. If the steamer ran aground the barge would immediately run into the steamer. Of course wider channels were required than if the barge were astern, and occasionally the channels were so narrow that the barges had to be dropped

astern in order to get them up. The twin screws had been tried on the river in three gun-boats. They went up the river and came down again, but he believed they had done nothing farther. Screws were not applicable to vessels of such shallow draught. As regards the holes in the plates, they were perfectly round. It seemed impossible that any simple corrosion could have caused them. Any description of jointed vessel would have the same difficulty as Bourne's; because when going round sharp bends, the vessel would go right into the bank. He did not believe any jointed system of barges could ever succeed upon the Indus, on account of the sharp bends, and the strong currents running round them. The barges were certainly a protection to the wheels of the steamer. As no one had said anything about his (Mr. Warren's) cargo steamer, he supposed there was no objection to it. With respect to disconnecting engines he certainly thought it would be desirable to try them on the Indus, for there might be circumstances under which it would be desirable, and an advantage to work the engines separately, but they must also be able to work them together. The *Stanley* was made with a full stern in order to get as light a draught as possible. It was a misfortune when the company had more steamers to make, that they persisted in making them of 2 ft. draught, because it was really less than necessary. There was no doubt that this style of stern was objectionable, because it caused a great commotion in the water. He thought there was a very fair chance for steamers making good profits upon the Indus. As regards the temperature of the water, it was in summer about 90 deg. in the middle of the day, and in the winter about 50 deg. Having explained the manner in which the barges were fastened to the steamer, he resumed his seat amid some applause.

The Chairman said, Mr. Warren had communicated a very practical paper upon a very important and interesting subject. It showed what had been done, and what had not been done, and he (the chairman) merely wished to say that Mr. Warren must not go away supposing his system was perfect for the navigation of the Indus, because no particular objection had been raised to it, he was afraid *that* arose more particularly from the want of information of the members on this subject. There must be one great drawback to the

GOV^T STE



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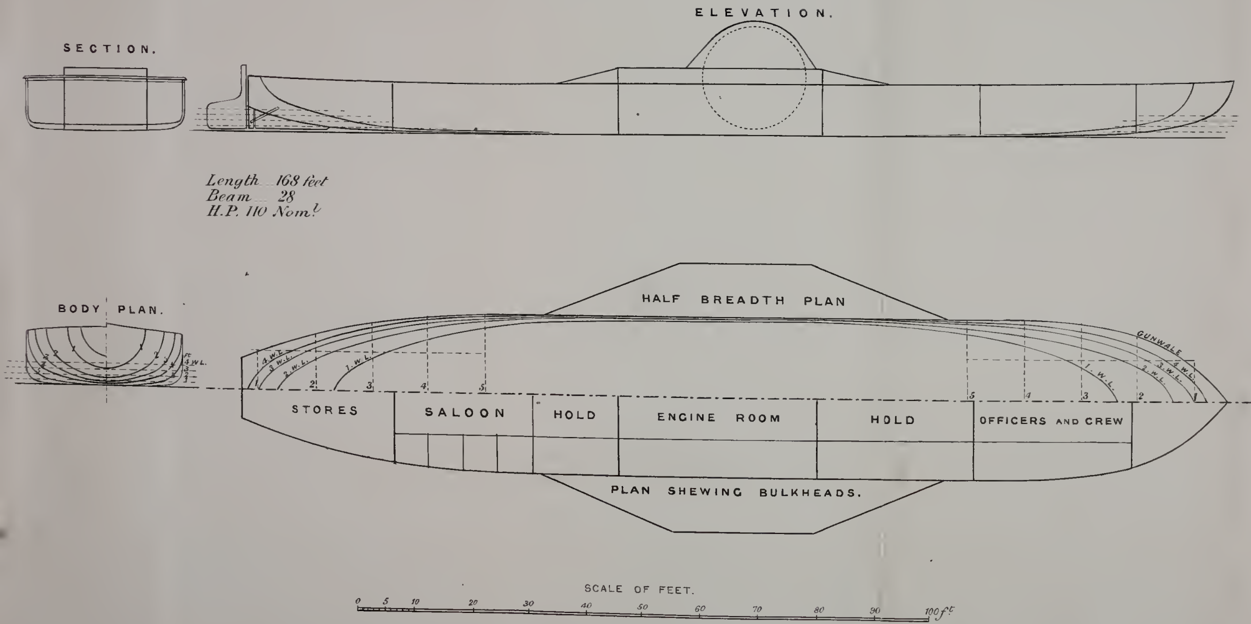
GOVT. STEAMERS.



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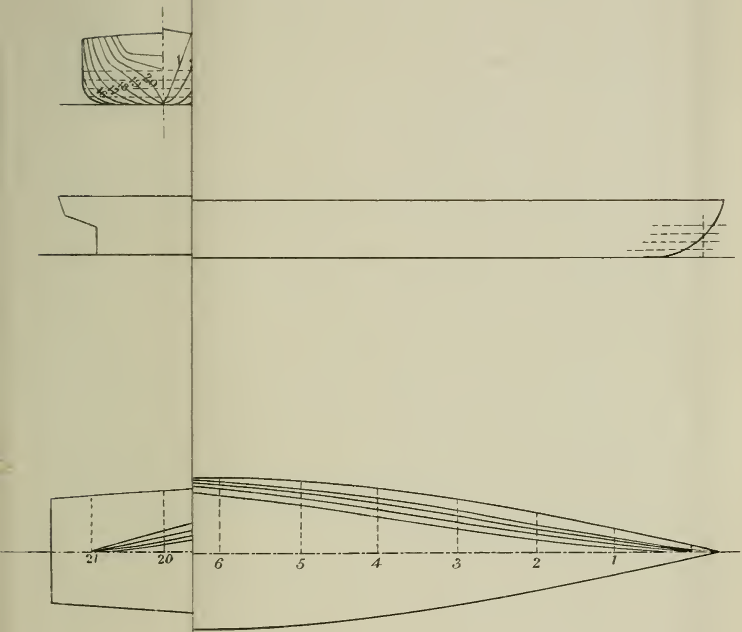

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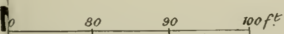

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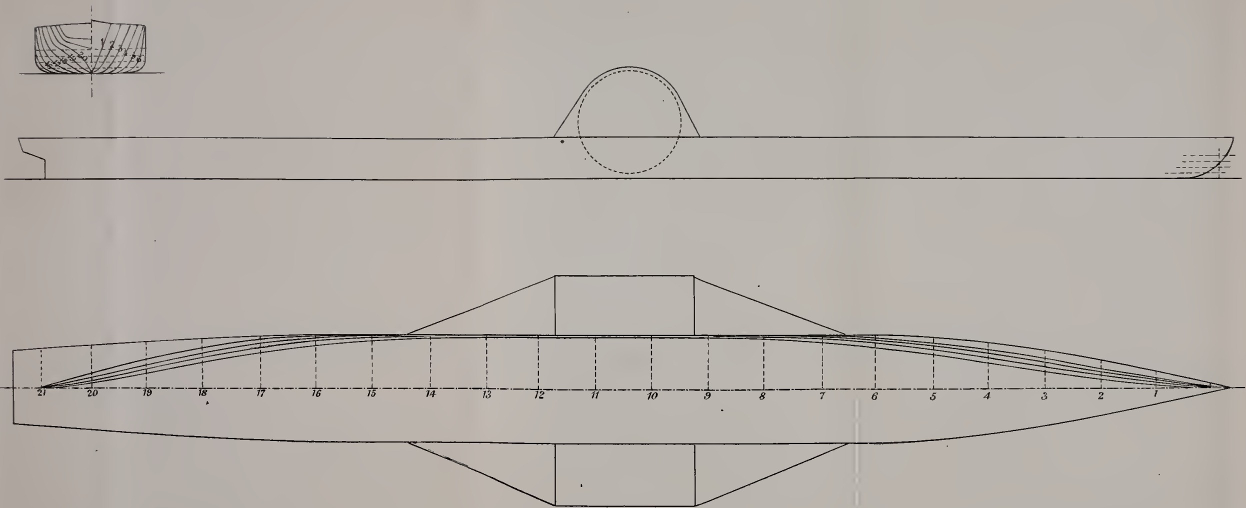

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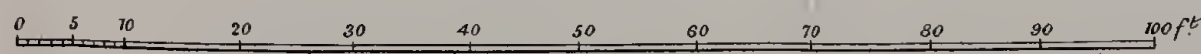
STEAMER,

"I N D U S".

SCALE OF FEET.



E AND F N. SPON, BUCKLERSBURY, LONDON.

Upfield Green, Litho

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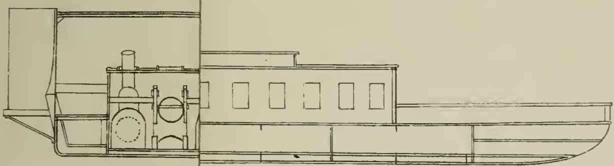
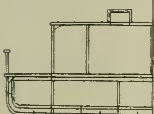
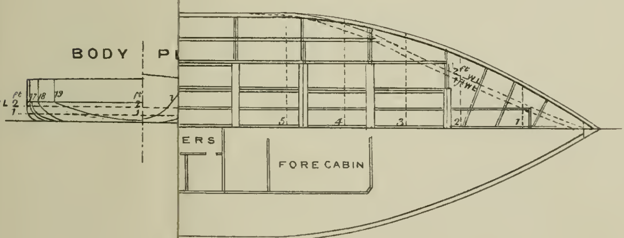
SECTION AT ENGI



SECTION AT



BODY P



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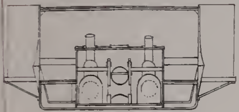
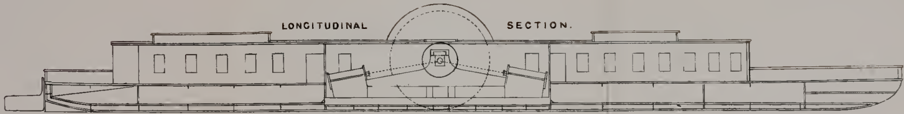
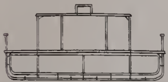
I. F. S. Co's STEAMERS.

SECTION AT ENGINE ROOM.



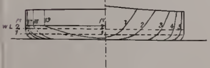


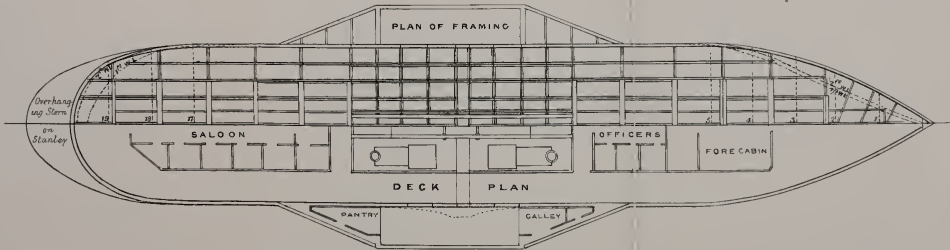
SECTION AT CABIN.



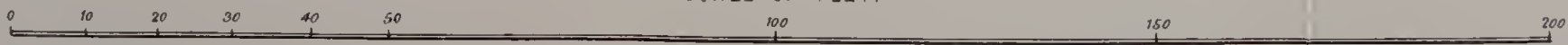
Length _ 200 ft
Beam ___ 38′
Depth ___ 6′. 6″
H.P. 120 Noml.

BODY PLAN.





SCALE OF FEET.



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Upfield Green, Lith.

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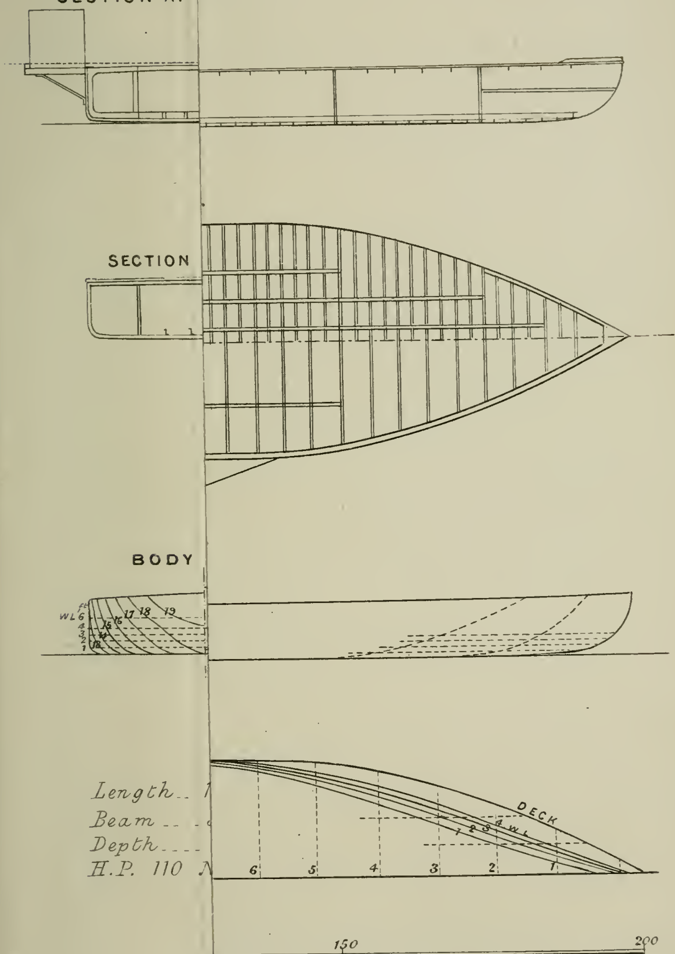
SECTION AT ꟼ N.
SECTION
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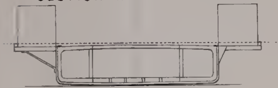
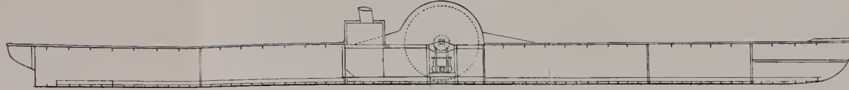
PROPOSED CARGO STEAMER.

SECTION AT ENGINE ROOM.

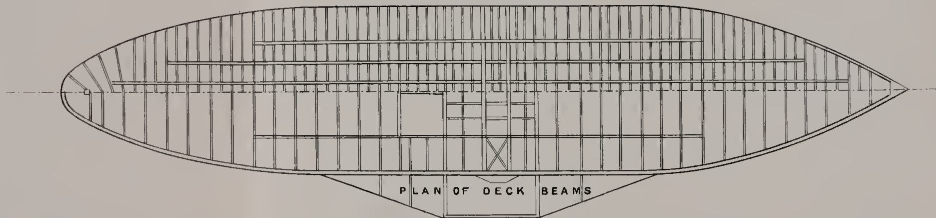


LONGITUDINAL SECTION.

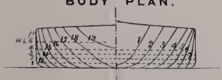


SECTION AT HOLD.

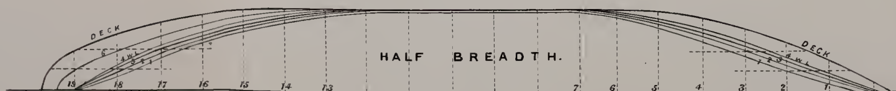
PLAN OF FRAMING.



BODY PLAN.

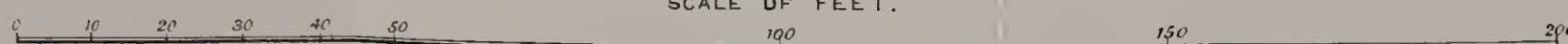


Length 190 ft.
Beam 38 "
Depth 9 6
H.P. 110 Nom.l



HALF BREADTH.

SCALE OF FEET.



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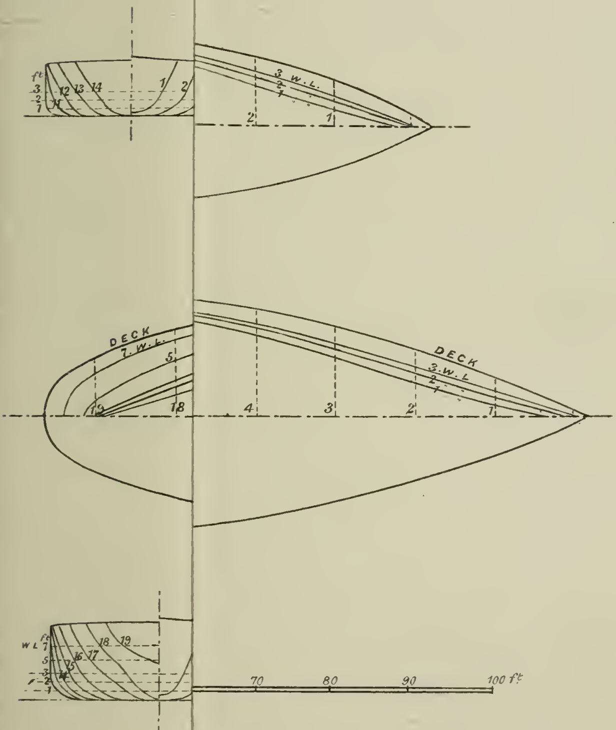
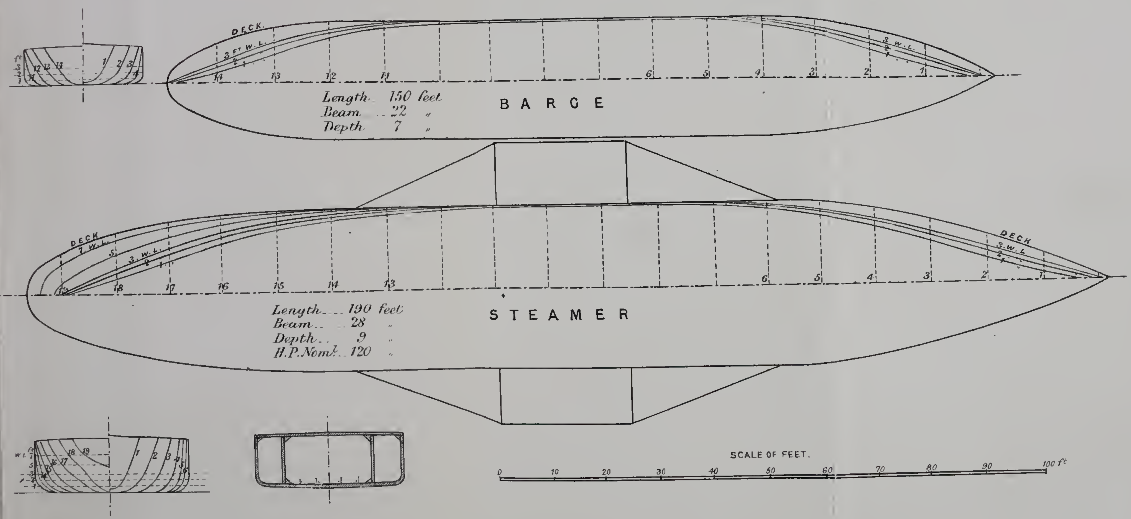


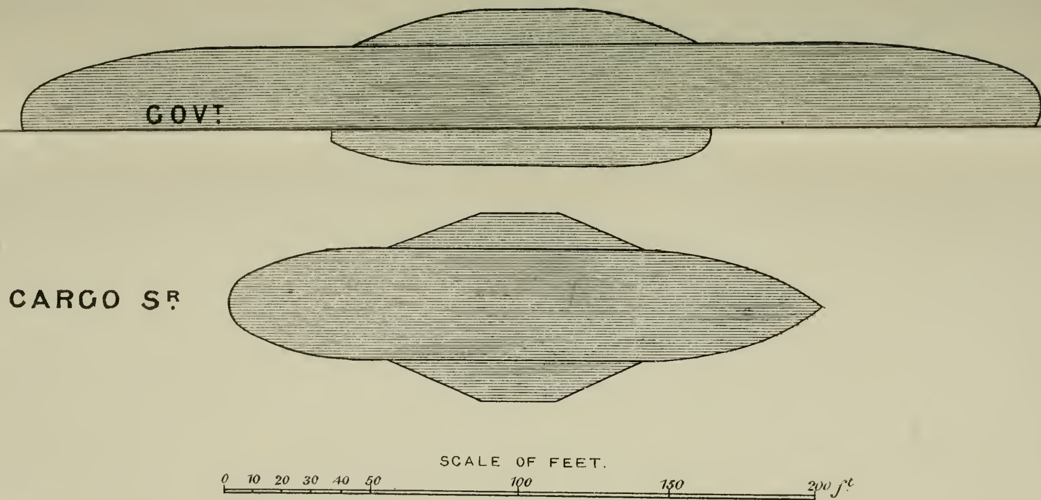
Unfield Green Litho

PROPOSED TUG.



E AND F. N. SPON, BUCKLERSBURY, LONDON.

Upfield Green Lith.



E AND F N. SPON, BUCKLERSBURY, LONDON

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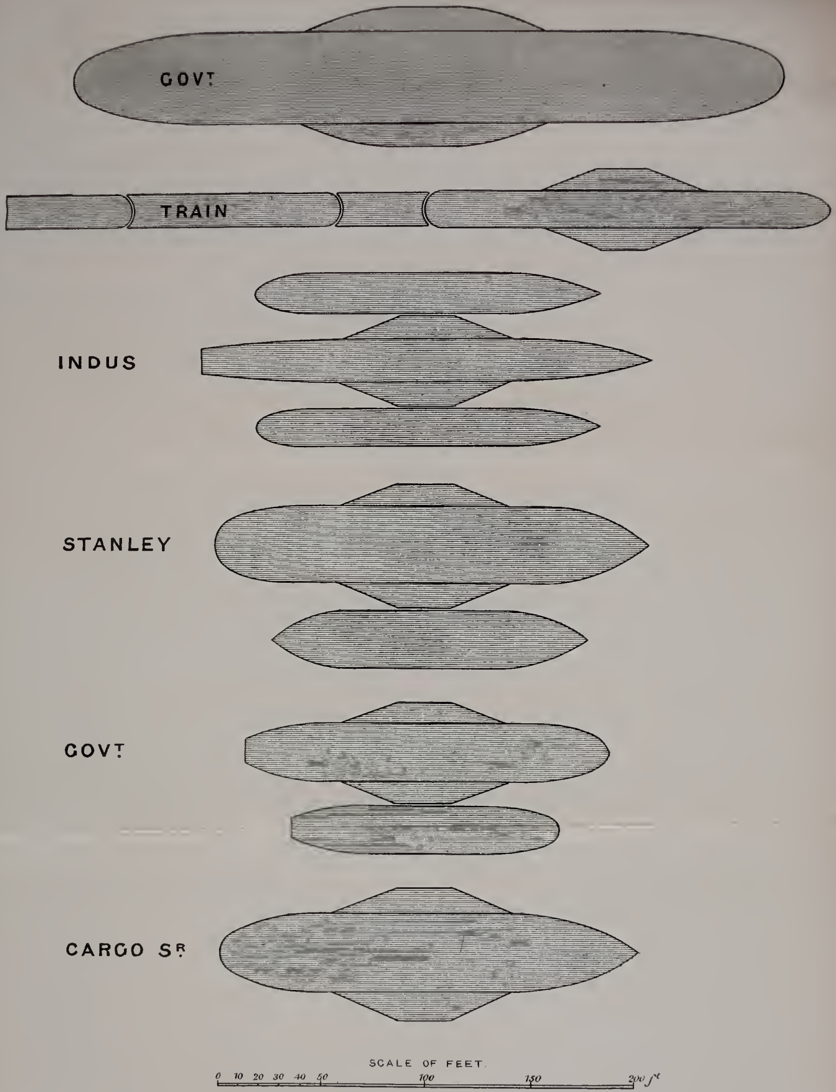
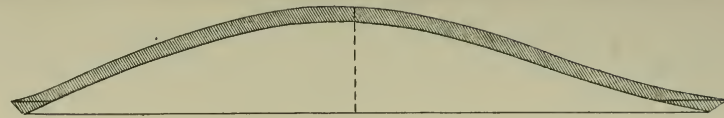
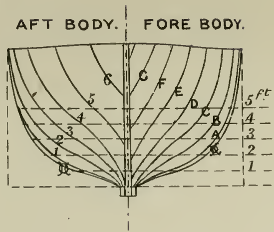
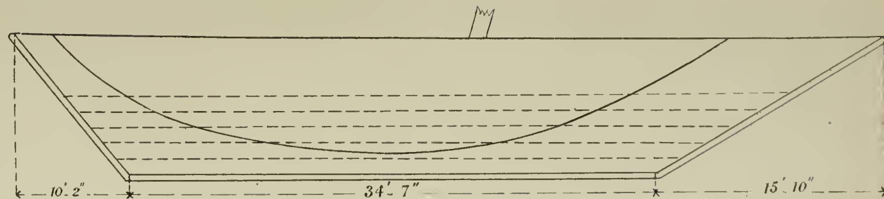


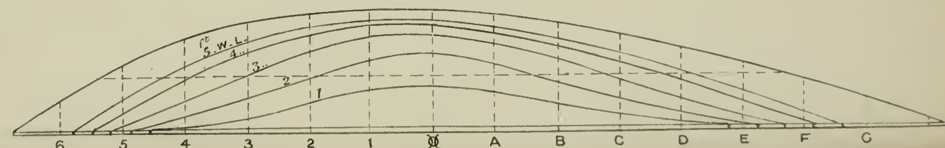
F AND F N SPON, BUCKLERSBURY, LONDON







Length of Deck — $60^{ft}.-7^{in}.$
— *d°.* — *Keel* — $34'-7''$
Beam — $15'-0''$
Depth — $8'-2''$



E. AND F. N. SPON, BUCKLERSBURY, LONDON

navigation of the Indian rivers, that was the want of commercia success; without this being overcome the navigation of the Indian rivers would remain an unaccomplished fact. He only hoped Mr. Warren would soon be placed in a position to enable him to carry out his views with regard to the best system of ship construction for the navigation of the Indus, and trusted, if so, he would meet with the success he anticipated.

October 5*th*, 1863.

R. M. CHRISTIE in the Chair.

STEAM FIRE ENGINES, AND THE LATE TRIALS AT THE CRYSTAL PALACE.

By W. Roberts.

In bringing the subject of steam fire engines before the Society of Engineers, it is unnecessary to enlarge upon the saving that must accrue from the use of steam, as compared with hand worked, or, as they are usually denominated, manual engines. It will suffice briefly to state that from carefully conducted experiments with two engines having pumps identical in every respect, one being fitted to a manual and the other to a steam engine, the work done by the steamer, with a consumption of $2\frac{1}{4}$ cwts. of coal, or say 2s. 6d., was from six to seven times as much as done for 30s. by the manual.

From the best information, it appears that the honour of constructing the first steam fire engine is due to Mr. John Braithwaite, who constructed one of about 10 horse power in 1829. Full particulars of it are not obtainable, but from the account of it in the *Mechanics' Magazine* of February 13th, 1830, it appears to have been capable of throwing 90 tons of water per hour, through one large or four small jets. Shortly after this Messrs. Braithwaite constructed another but smaller steam fire engine. The

account of it as given in the same number of the magazine says: "Shortly after the conflagration broke out, to which the Argyle Rooms fell a prey on Friday last, a new fire engine worked by steam, on the same principle as the 'Novelty' steam carriage, and manufactured by the same ingenious and enterprising engineer, made its appearance on the scene, under the direction of Mr. Alfred Braithwaite, and earned for itself universal admiration, by the powerful services which it rendered on the occasion."

It was worked incessantly for nearly five hours; discharged upon the burning pile and adjacent buildings from thirty to forty tons of water per hour, and sent its jet of water completely over the dome of the building, a height of at least eighty feet.

Several gentlemen who were present at the fire have stated that they distinctly saw the water ejected from Mr. Braithwaite's engine quite over the dome, and one of the same individuals previously witnessed a private trial of the engine when it threw the water over a pole 90 ft. high, erected for the purpose of the experiment. The expenditure of fuel to produce these wonderful effects was only about three bushels, during the whole five hours, two men being sufficient to keep the steam up and superintend the working of the engine.

(Plate No. 1.)—The following is the description of the engine: In all that regards the generation and application of steam, the same contrivances are adopted in this engine as in "The Novelty;" but there is a difference in the form of some of the parts and in the general arrangement.

The furnace and boiler K, are exactly the same as those of "The Novelty," but of less power. The hot air pipe, instead of being vertical, has received a serpentine form, B, which adds much to the appearance of the engine in point of elegance and compactness. This pipe has also been made to turn on a swivel, so that the engineer or driver of the carriage can turn the mouth of it in any direction which he finds most convenient. The blowing apparatus is here placed in front under the driving box, and is worked by the tappet lever C. The cylinders M, are placed horizontally, and the steam piston is connected with the water pump (E) plunger by one rod working through the two stuffing-boxes, so as to form its own parallel motion. A

crosshead attached to the piston rod, sliding on the frame which supports the two cylinders, works the tappet lever C, connecting with the slide D, the feed pump F, and the blowing apparatus. The feed pump and blowing apparatus have also separate appendages for working them by hand occasionally, and provision has been further made for regulating the stroke of the former, according to the work of the engine. P is the mercurial gauge, R the safety valve, S the feed box to the furnace, and T the eduction or waste steam pipe. A box O, serves to hold the coke or other fuel, and as a platform for the assistant engineer.

The steam cylinder is 7 in. in diameter; the length of the stroke of the piston 16 in.; the number of strokes per minute from 35 to 45; the power barely 6-horse.

The parts of this apparatus peculiar to it as a fire engine are as follows: The globe or sphere A, is the air vessel; the water pump E, is $6\frac{1}{2}$ in. in diameter; the oblique pipe, G, G, is one of the suction pipes. The action of the water pump, being double these suction pipes, either connect with the tank H, or in the common way at J. The object of having the tank H, is to enable other engines to supply this one with water in situations—such as narrow streets and alleys —where a supply of water close at hand cannot be obtained. But it must not be supposed from this that there is any necessity for placing this engine in a particular situation, in order to connect it directly with the water main. On the end of its suction pipe is a common perforated rose, and it may lie in any part of the water way, with the suctions of other fire engines, and take in supply from the same source. It worked with one jet only at the late fire, but is constructed to operate with one or two jets, as occasion may require. The total weight of the machine, with its complement of fuel and water, is incredibly small, not more than 45 cwt.

In 1832 Mr. Braithwaite constructed a third steam fire engine, called the "Comet," for the Prussian Government. The boiler was constructed on the same plan as the "Novelty's," except that combustion was promoted by an exhauster instead of a blower. The steam cylinders, two in number, were 12 in. diameter with a 14 in. stroke, and two water cylinders, $10\frac{1}{2}$ in. diameter, also 14 in. stroke.

The steam from the eduction pipe passed through two coils of

tubing laid in the feed water tank, thereby raising the temperature of the feed water.

The steam was got up to 70 lb. per inch in twenty minutes; and, with $1\frac{1}{4}$ in. jet, the water was ejected to a height of from 115 ft. to 120 ft., the engine making eighteen double strokes per minute.

The weight of this engine was about four tons, and the consumption of coke about three bushels per hour: a few months back, a gentleman who had recently returned from Berlin stated that it was still in existence, although it was now employed to pump the water from a mine.

Although Messrs. Braithwaite built two more engines they did not succeed in getting them generally introduced; and it does not appear that any more steam fire engines were made in England for upwards of twenty years.

In the meantime a Captain Ericsson, a Swede by birth, and who had been for some time with Messrs. Braithwaite, went to America, and evidently remembered what he had seen in this country; for upon the Mechanics' Institute of New York, in 1840, offering a gold medal for the best plan of a steam fire engine, he obtained the prize.

The boiler of this engine was of the ordinary locomotive type, and contained twenty-seven tubes $1\frac{1}{2}$ in. diameter, and a blowing apparatus was used to fan the fire, very similar to Braithwaite's first engines. The arrangement of the steam and water cylinders was also very similar to Braithwaite's.

About the same time Mr. Paul R. Hodge built an engine for an association in New York, in which the steam power was used as an auxiliary to propel it.

About 1850 Mr. A. B. Latta, of Cincinnati, constructed an engine similar to the above, but weighing from ten to twelve tons. The principal peculiarity of Latta's engines are the boilers; these consist of a rectangular fire-box, open at bottom, and having a light iron top. In this fire-box, immediately over the fire, are sections of tubes, all connected to a pipe at the bottom, and coming into the top of the fire-box. This pipe is connected to a circulating pump, which pump is also connected to the bottom of the fire-box. Upon lighting the fire with but a small quantity of water in the fire-box the tubes quickly become hot. The circulating pump is then started by hand. The

water being drawn from the lower part of the fire-box, is forced into the hot tubes, and steam is thereby quickly formed. Indeed it is stated that sufficient steam is got to start the engine in from three to six minutes, but from the experience the author has had with one, he concludes the donkey engine that works the circulating pump must be meant, for he never yet had steam to work the main engine under fifteen or twenty minutes.

From this time the steam fire engine made very rapid progress in America; fire companies were formed, and these, being taken under the protection of the state, having certain privileges granted them, a spirit of emulation soon sprang up amongst them; each became anxious to have the best engine at any cost, and, as a natural consequence, some of the most eminent engineers found it worth their while to take the matter in hand, and the result was that our cousins across the water had some very excellent steam engines whose puff puff, could be heard at their fires, whilst we, with our old-fashioned notions, could only hear the old bump, bump, relieved now and again with the cry of "beer, oh!"

Up to 1852 the most powerful engines in London were two floating engines, each having two 9 in. water cylinders (single-acting) on each side the boat; but in that year the authorities appear to have discovered that it was rather expensive to work these machines by hand; one of the floats was, therefore, placed in the hands of Messrs. Shand and Mason; the hand levers were removed, boilers were put in, and a steam cylinder erected over each water cylinder, the connecting rods having slotted cross-heads; in these cross-heads crank shafts were placed, the cranks being opposite each other. On one end was a fly-wheel, having a ratchet on one side to get a lever to work to pinch it over the centre occasionally. The steam cylinders were double-acting, and as the pumps were single-acting, the one cylinder upon its up-stroke assisted the other through the crank shaft.

At the same time arrangements were made to carry the water aft to propel the boat upon the reaction principle, but this was a miserable failure, as it scarcely gave the boat steerage way.

The saving effected in working this float was, however, so satisfactory that, two years later, the committee decided to have a boat built and fitted expressly as a steam fire engine.

Many disparaging accounts of the first floating steam fire engine had been given, but the author had seen it at work several times, and it did very good work indeed, and considering that it was only converting a hand into a steam-worked engine, it was as successful as could be expected, always excepting the propelling.

The second—or, as it is styled, the upper float—was built by Mare and Co., of Blackwall, the length being 130 ft.

The engines, pumps, &c., were made by Shand and Mason, the steam cylinder being 14 in. diameter, with an 18 in. stroke, the pumps (double-acting) being 10 in. diameter.

The engines were placed horizontally one on each side the vessel, and a large Appold pump was placed in midships to propel the boat upon the reaction principal. The engines were arranged so that they could be readily thrown off the pumps when wanted for propulsion, but, by what seems to have been an oversight, only one could be thrown out of gear at the same time with the propelling pump; thus a considerable amount of power was lost, but even in this state it was capable or working four 1½ in. jets at one and the same time. At the great fire in Tooley-street it worked nearly 400 hours.

The engines are nominally of 80 horse power, but are frequently worked up to double that power.

During the present year some alterations have been made in the mode of propelling the boat, the outlets have been brought above the water's edge, and arranged something upon the Ruthven plan, and although a decided improvement, the boat is still very very slow for the amount of power consumed.

About this time the East and West India Dock Company had their steam tug *Dragon* fitted with a large Downton pump, to be worked by the boat's engines. This is a very effective engine, delivering a 1½ jet to a distance of 160 ft. to 180 ft.

In 1860 Messrs. Merryweather and Sons constructed from the designs of Mr. Edward Field, a member of this Society, an engine for the Tyne docks. The performance of this engine was very satisfactory.

About the same time the author fitted for the East and West India Dock Company, on board their tug *Lucy*, a pair of his patent double action twin pumps (of the same construction as those he has the

honour of supplying for the use of Her Majesty's Navy) each pump had two cylinders of $11\frac{1}{2}$ in. diameter × 12 in. stroke; the collective discharge being equal to a column of water $11\frac{1}{2}$ in. diameter × 8 ft. high, or thirty gallons at each stroke; these were the largest fire engine pumps he knew of, they had once been used at a fire, and then they worked so satisfactorily that he was informed by the chairman of the company that they had paid for themselves.

In May, 1856, the late Superintendent of the London Fire Brigade read a paper at the Society of Arts, and during the discussion that followed, Mr. Braithwaite, smarting no doubt from the treatment he had experienced, roundly charged the Fire Insurance Offices with not being anxious that fires should be extinguished.

The answer of Mr. Braidwood, goes far to show the reason why steam fire engines were not brought into public use earlier. He said "he could also state, that the subject of steam fire engines had been seriously considered, but if the fire offices were disposed to incur the expense or cost which six or eight of these engines would involve, there was not at the present time an adequate supply of water from the mains to work any number of them at a fire. There was scarcely a sufficient supply for ten or twelve of the ordinary engines now in use; therefore steam fire engines, admitting their great efficiency, were out of the question until a larger supply of water could be obtained. If he might compare large matters with small he would compare the steam engine to a battering train, most efficient when well placed and served, but slow of movement, and requiring large supplies."

"The common fire engine, well constructed, and not too heavy, might be likened to field artillery, which, from its power, but especially from the rapidity of its movements, was invaluable.

"To carry out the simile, hand pumps might be supposed to take the place of musketry, which might be used when and where it was desired; from a washhand basin, if necessary.

"With regard to the water supply his impression was that, when they came to have an adequate supply for steam fire engines, a higher pressure would be given, so that they could work from the main without requiring the steam fire engine." (See *Journal of the Society of Arts*, May 23, 1856, p. 459.)

Shortly after this, in a conversation the author had with Mr. Braidwood, he offered to construct a steam fire engine, to be drawn by two horses; and subsequently discussed with him the details of boiler and engine; but he would not hear of it. In fact, when he constructed his engine "True Blue," and offered it to him for twelve months, he told him a large engine like that was worse than useless, as, if they wanted to deliver 180 gallons from one jet, they had only to marry two ordinary engines to one branch pipe. He lived long enough to acknowledge that it was not worse than useless, and to see it do good work repeatedly; also to pen the following sentence: "That at large fires beyond the reach of the steam floating engine, the land steam fire engine has been of great service. It is not only the large quantity of water it throws, but the height and distance to which it is thrown, that makes it so valuable; at the same time it can be worked as gently as an ordinary engine."

(Plate No. 2.)—In 1858 Mr. James Shand obtained letters patent for a steam fire engine, and one was constructed in that year, and publicly tried at the Grand Surrey Canal, Camberwell, on the 14th October of the same year.

In 1860 Messrs. Shand and Mason constructed an engine which was used by the London Fire Brigade for some months, and is the one referred to by Mr. Braidwood in his report previously referred to. This engine worked for something under twelve months, and was then withdrawn, its last public performance being at the great fire in Tooley-street. The specification describing it, says, "According to this invention the steam cylinder which actuates the pump is inverted, and situate over the air-vessel of the pump, which is made double-acting, one barrel being placed above the other, and a double or two-throw crank is placed between them. One or both of the pistons or plungers of the pump is fitted with a valve, and the piston-rod of the steam cylinder is connected directly with the piston of the upper pump-barrel, which latter serves as a guide to the piston-rod of the steam cylinder. A connecting-rod from the upper pump piston connects it with the crank, while a second connecting-rod connects the piston of the lower pump barrel with its throw of the crank. The slide valve of the steam cylinder is worked from eccentrics on the crank-shaft outside the pumps, and the lower

pump barrel is enclosed in a suction air vessel, fitted with a separate valve, if necessary. A fly-wheel on the crank shaft and a feed pump are placed near the boiler. The whole is supported on a carriage, consisting of a suitable framing running on travelling wheels, and furnished with springs and locking carriage. In the centre of the hind axle is placed an upright boiler, the steam pump being situate vertically between the the front and hind axles, and behind the driver's seat. Beneath the seat is placed the hose-reel or a box for containing the hose and implements."

The next steam fire engine supplied by Messrs. Shand and Mason for the use of the London Fire Brigade is now stationed at Tooley-street, and was supplied in April, 1861. This is very similar in its general arrangement to Braithwaite's, except that it has a slotted crosshead and fly-wheel, with an eccentric to work the slide instead of the tappet; the following description of this engine is taken from the report of Mr. Lewis M. Becker to F. Hodges, Esq., at whose place this engine was tried, the dimensions, &c., of several other engines have also been obtained from this source.

"Steam Fire Engines constructed for, and now in use by the London Fire Engine Establishments, Shand and Mason, inventors and manufacturers, 75, Upper Ground-street, Blackfriars (late of 245, Blackfriars-road), London.

"The engine 'No. 5' (Tooley-street Station, London Fire Engine Establishment) and the Engine 'No. 10' (Wellclose-square Station, London Fire Engine Establishment) are similar in construction though varied in outward form. They are fitted with high wheels and boxes to contain hose and implements, upon which the firemen ride; on the rods at the sides the suction hoses are suspended. The following description will serve for both engines:

"The extreme outside dimensions are: Length, 13 ft.; width, 6 ft. 5 in.; height, 9 ft. without chimney. The total weight, as made up for running, including coke in bunkers, is 2 tons 14 cwt. 51 lb.; the weight of water in boiler is 3 cwt. 64 lb. extra. These engines, similar to the manual manufactured by this firm, can draw water from the cistern which forms a part of the engine.

"The single steam cylinder is 8½ in. in diameter, the piston-rod of

which is fixed to the piston of a double-acting pump 7 in. in diameter —both pistons necessarily having the same stroke, which is 9 in. The pump is composed of gun-metal and copper, the valves being India-rubber, as used in the steam floating fire engines of the London Fire Engine Establishment. The whole of the machinery is of a very simple construction, and is not liable to be affected by rapid transit over rough roads.

"The boiler is of the upright tubular construction, affording means for superheating the steam; it contains 199 brass tubes, each of 1¼ in. outside diameter and 15 in. in length. The fire-box is composed of copper, and is 3 ft. 4 in. in diameter. This boiler is, in fact, the one used in the engine last described.

"*No.* 5 *Engine.*—Steam was raised from cold water to a pressure of 30 lb. in 12¼ minutes; 40 lb. in 13 minutes; 50 lb. in 14 minutes; 60 lb. in 14½ minutes, and 70 lb. in 15 minutes. The engine was then set to work through 210 ft. of hose, a jet of 1¼ in. diameter being used, and a vertical height of 160 ft. was reached. A considerable breeze was blowing. With the same jet pipe and length of hose a horizontal distance of 202 ft. was reached. The engine was continuously worked with the greatest smoothness and regularity for two hours, drawing and delivering 440 gallons of water per minute.

"*No.* 10 *Engine.*—At the trial of April 16th the following results were obtained: Steam was raised from cold water to a pressure of 60 lb. in 14½ minutes. The steam gauge moved in 6 minutes from lighting the fire, and 10 lb. of steam was raised in 10½ minutes. A jet of 1¼ in. diameter was thrown a height of 174 ft. (34 ft. over chimney), working under steam pressure of 120 lb., and of water pressure 140 lb. A ⅞ in. jet was thrown horizontally a distance of 187 ft.; with an inch jet, 196 ft.; and with a 1¼ in. jet, 190 ft. A tank, capable of containing 448 gallons, was filled with two deliveries, each of 1 in. in diameter, in 1 minute 15 seconds; on this trial being repeated the same results were obtained. The average pressure of steam was 120 lb., and the water pressure 140 lb. A strong wind was blowing the whole of the time.

"In 1861 Messrs. Merryweather constructed a steam fire engine, which they called the 'Deluge,' of which the following description is their own.

"THE 'DELUGE,' Merryweather and Son, and E. Field, C.E., inventors and manufacturers, 63, Long Acre, London.

"This engine, mounted on a strong wrought iron frame, is, with hose, coals, water, &c., 3½ tons weight. The boiler is made of steel plate; a large heating surface is obtained by a quantity of vertical tubes, and the upper part of the boiler or steam chest is fitted with strong wrought-iron tubes for carrying off the smoke and creating a draught, which act as strong stays; the outer water-jacket is also well and frequently stayed, so that altogether it is very secure for high pressures. The internal arrangement of the boiler is such that a perfect circulation of water is carried on, and no deposit remains in the tubes, as the rapid circulation throws any foreign matter over into the outer jacket. As the copper tubes are not fixed at their lower ends, and the upper tubes are of iron, no unequal expansion or contraction of metals take place, and leakages are avoided. The boiler is fitted with every requisite for its safe working, viz., two safety valves, gauge-cock, gauge-glass, pressure-gauge, blow-off cocks, &c. It is fed by a Giffard's Injector, which feeds itself from a tank that is self-supplied from the pump; the boiler can also be fed from the main pump if required. It is felted (to prevent loss of heat by radiation), and covered with sheet iron and brass bands. Steam is taken from four points, and supplied direct through the valve-chest into the cylinder, passing in its way under the cylinder. This class of boiler does not prime.

"The steam cylinder is 9 in. in diameter, with 15 in. stroke of piston; no fly-wheel is used, and, by the valve arrangement, a uniform speed of piston throughout its stroke is produced, which has great effect in working the pump, as an unusually steady column of water is obtained, and the engine can be started at any point by opening the steam-valve; and by this arrangement the engine can be made to run at any required speed, from 1 or 2 up to 150 or 160 double strokes per minute. The piston-rod is coupled direct to that of the pump, and two stout guide-rods connect the pump with steam cylinder, the rods taking the whole of the strain.

"The pump is horizontal, double-acting, and 6½ in. in diameter; the piston makes the same length of stroke as the piston of the steam cylinder. The whole of the valves, both suction and delivery, are placed beneath the cylinder. The advantages of this arrangement are

that no grit or foreign matter which may be taken up with the water can injure the pump-cylinder, and the water passages are so arranged that any foreign matter which may find an entrance may be discharged from the pump, and that they may be secure against accident from freezing.

"The internal diameter of the suction-hose is 5 in., and of the delivery hose 3 in.; the piston of the pump is so arranged as to lubricate itself with oil at every stroke. The three trials of this engine produced the following results: May 24th, steam raised from cold water, 48 deg. Fah., to a pressure of 40 lb. in 10 minutes from lighting the fire. The engine was then started, and, with a $1\frac{1}{4}$ in. jet, a column of water was thrown 30 ft. over the chimney shaft 140 ft. high, making a total height of 170 ft.; subsequently, with a $1\frac{3}{8}$ in. jet, a height of 157 ft. was gained, and with a $1\frac{1}{2}$ in. jet, a height of 150 ft.; with a $1\frac{1}{4}$ in. jet, a distance of 215 ft. was gained in a horizontal direction, and, with a $1\frac{3}{8}$ in. jet, a distance of 202 ft.; it then filled a tank capable of containing 448 gallons in one minute eleven seconds, working through a $1\frac{3}{8}$ in. nozzle. The average steam pressure was 120 lb., pressure on pumps 100 lb., and strokes 120 per minute. Wind fresh, S.E. S."

The trial of November 22nd, and 24th are similar. Cold water being used, 44 deg. Fah.

	h.	m.	s.	lb.
Fire lighted at	2	0	0	0
Steam gauge moved	2	7	30	0
Steam pressure at	2	8	30	10
Ditto ditto	2	8	50	15
Ditto ditto	2	9	30	30
Started pump at	2	9	50	50
Steam pressure at	2	11	0	75
Ditto ditto	2	12	0	90
Ditto ditto	2	13	0	120

With a $1\frac{1}{4}$ in. jet vertical . .	170	feet was reached.
Ditto $1\frac{3}{8}$ ditto ditto . .	165	ditto
Ditto $1\frac{1}{2}$ ditto ditto . . .	155	ditto
Ditto $1\frac{1}{4}$ ditto horizontally .	220	ditto
Ditto $1\frac{3}{8}$ ditto ditto . . .	210	ditto
Ditto $1\frac{1}{2}$ ditto ditto . .	194	ditto

300 ft. of hose was led over the distillery buildings up the fire observatory (a perpendicular height of 140 ft.) and with a $1\frac{1}{2}$ in. nozzle, the engine threw from 75 ft. to 100 ft. horizontally, working under 140 lb. steam pressure, and 160 lb. on the pump, running at a rate of 120 strokes to the minute. Wind very fresh, E.N.E.

About the same time he (Mr. Roberts) constructed the first self-propelling steam fire engine in this country, if not in the world, for the American self-propelling engines, in all cases that he had heard of, had to be assisted by horses or men, particularly in steering. The author's engine was also arranged for driving machinery, or for a hoist. (Plate No. 3.)

The extreme length of the engine is 12 ft. 6 in., and the extreme breadth is 6 ft. 4 in. Its gross weight, with 5 cwt. of coals, 40 gallons of water in the tank, suction hose, delivery hose, ladders and tools complete, is $7\frac{1}{2}$ tons. The engine has Benson's forced circulation boiler, in which the water is contained within several series of parallel tubes, laid horizontally, and connected with a large steam-proof vessel called the receiver. Originally a constant circulation of water was maintained through the tubes, and between the tubes and "receiver," by means of a small donkey-pump called the circulating-pump, which is independent of, and additional to, the force pump. The author has since found it advantageous to alter this arrangement materially. The steam is worked through a pair of vertical steam cylinders, 6 in. diameter, each of which has a stroke of 12 in. The diameter of each water cylinder is $9\frac{1}{2}$ in., with a stroke of 8 in. Power may be applied at pleasure to either or both of the driving wheels, which are 5 ft. in diameter, pitch chains being employed to get up the speed. The engine was propelled along the public road at a rate of 18 miles per hour. By working only one of the driving wheels, which are hung on springs, and with the aid of a single steering wheel in front, of 3 ft. diameter, the engine was turned within a circle of 12 ft. in diameter. On the engine shaft is a pulley or rigger, and a windlass, the former being used for driving machinery of almost any kind, and the latter as a hoist. The fire engine pumps are a pair of the author's patent double-acting pumps, $9\frac{1}{2}$ in. in diameter each, and fill for 8 in. of the length of the barrel at each double stroke of the steam pistons.

At the trial on September 12th the following results were shown: With a $1\frac{3}{8}$ in. jet a column of water was thrown over the chimney shaft, which is 140 ft. in height, and, with the same jet, a horizontal distance of 182 ft., exclusive of broken water and spray, was reached. By forming a connexion, whenever required, between the suction chamber and delivery chambers of the pumps, a $\frac{3}{4}$ in. and a $\frac{1}{2}$ in. jet can be used. With a jet $\frac{1}{64}$th of an inch a quart measure was filled, which occupied 12 minutes 30 seconds, the engine working under a steam pressure of 120 lb., and 100 strokes per minute; average pressure of steam at the trial, 50 lb. to 160 lb.

The boiler at the date of the trial was very defective, but several improvements have been made in it, and the last trial shows 150 lb. steam pressure was obtained in 19 minutes 45 seconds after lighting the fire (all coal). The fire was then let down, and the steam pressure reduced to 70 lb. The engine was at this time working at 40 to 45 strokes per minute, and, in ten minutes from lowering the pressure, the steam had risen to a pressure of 170 lb., and the strokes to 112 per minute, when it had to be checked in consequence of a deficiency of water to supply the pumps, which were delivering at this time at the rate of 450 gallons per minute.

Since this time the boiler had been very much improved, so much so that there was none of that fluctuation so much complained of in fire engine boilers in general, and the American in particular.

This engine was taken one morning to Her Majesty's Dockyard, Woolwich, over the road that had just been opened for the main drainage, and which was so soft that it repeatedly sunk to the foot-plate, and after working all day returned at night.

Upon one occasion the machinery of the factory of Messrs. Brown and Lennox, of Millwall, was driven with this boiler; the bottom of the boiler gave out, and stopped the engine. He had the propeller taken up alongside, and made a connection from the boiler to the engine, and started the next morning first thing. The cylinders were two $11\frac{1}{2}$ in. diameter, with 23 in. stroke, and the usual pressure with the proper boiler was 30 lb., but the steam fire engine had to work with 80 lb. in the boiler.

We have now arrived at the time when the first American steam fire engine made its appearance among us. This was a small engine,

made by Messrs. Lee and Larned, of New York. These gentlemen were among the foremost of steam fire engine builders in America, and came over with their little engine for the International Exhibition, and which was first tried at the Lambeth Distillery, the following account is taken from the report before referred to:

Steam Fire Engine Trials, Lee and Larned, Inventors and Manufacturers, Novelty Ironworks, Brooklyn, New York.

This engine, of a light, elegant, and strong construction, is with the exception of the boiler tubes (which are of wrought iron), composed of steel of the best brand. The engine itself, which has neither water tank nor coal bunkers, weighs 1 ton 12 cwt. The cylinders, which are 7 in. in diameter, with an 8½ in. stroke, have a pair of light balance wheels, to carry it over the centres; the piston-rod is 1¼ in. in diameter, the crank shaft 1¾ in. in bearing, the boiler 4 ft. high by 26 in. outside diameter at steam chest. The engine, which is single reciprocating, works very smoothly indeed: and, though running at the rate of 400 revolutions per minute (full power), scarcely any vibration is visible. The pump, which is fastened to the bottom of the cylinder, is Cary's patent rotatory, very highly finished; the valve movement is obtained by means of a rock-shaft, actuated by an eccentric rod from the main shaft. The boiler (Lee and Larned's patent annular) has 125 ft. of heating surface, and is supplied by a Giffard's feed-water injector; it has also a connexion with the main pump. The upper steam chest (double-riveted ¼ in. sheet plates) has a flat top and bottom, resting on tubes fastened to the bottom of the same; these tubes encircle the whole of the interior of the boiler. The arrangement of the carriage springs (invented by J. K. Fisher, of New York) is very beautiful; the centre of the load is directly over the hind axle, and the engine will stand any amount of jolting over rough roads, without any strain of the engine proper. A tender, at will, is attached behind, to carry the hose, coals, &c. At the trials of February 4th and March 24th the following results were obtained:

Feb. 4th, 1862.	h.	m.	lb.
Fire lighted at	3	58	0
Steam gauge moved	4	3	0
Steam pressure at	4	5	5

Feb. 4th, 1862.	h.	m.	lb.
Steam pressure at	4	8	12
Started pump at	4	9	15
Steam pressure at	4	9½	20
Ditto ditto	4	10	30
Ditto ditto	4	10½	40
Ditto ditto	4	11	65
Ditto ditto	4	11½	90
Ditto ditto	4	12	120
Ditto ditto	4	13	135
Ditto ditto	4	15	140

At this time it threw, through an inch nozzle, a stream of water to a vertical height of 135 ft.; 4.30 P.M., working with a pressure of 145 lb. to 150 lb. of steam to the square inch, it threw, through an inch nozzle, a stream 197 ft. horizontally, and with 160 lb. to 170 lb. of steam, it threw a 1¼ in. jet horizontally 194 ft. Wind rather fresh. S.S.W.

March 24th. Steam was raised from cold water 48 deg. Fah. 3½ minutes after lighting the fire. In 7½ minutes from the time of lighting, the engine was started slowly, with 5 lb. pressure. In 15½ minutes it was working with a pressure of 145 lb. to 150 lb., throwing a 1⅛ in. jet 150 ft. high (10 ft. over the chimney shaft), and afterwards two 1 in. jets 120 ft. high. It threw an inch jet horizontally 200 ft. Its best single stream for distance is an inch jet, for quantity a 1⅛ in. jet; but for ordinary fire duty it will throw, with good effect, two 1 in. streams, with an average number of 300 revolutions per minute.

In April, 1862, Messrs. Shand and Mason supplied the Engine "No. 10," formerly described, to the London Fire Brigade, and this brings us up to the first competitive exhibition. Hitherto, Messrs. Shand and Mason had the whole run; but, by some means, Messrs. Merryweather had got permission to put their engine, the "Deluge," into the Exhibition, and the author thought the behaviour of the authorities of the Exhibition was not satisfactory. Messrs. Shand and Mason, and Messrs. Merryweather and Co.'s exhibitions commenced at the main entrance, and were continued all over the build-

ing, and the engines of all other makers were stowed away in a sort of lumber room at one corner of the building, under one of the towers. Even after the awards were made the author repeatedly had letters to know where the engines were placed, as they could not be found.

After the trials of hand engines, and when the "Deluge" was ready, the sub-committee decided to have the steamers tried; but for reasons best known to themselves, Messrs. Lee and Larned declined to have their engine tested or tried. The author was in difficulties with the Benson boiler in his engine, and had not the London Fire Engine Establishment lent the Engine No. 10, and the one made by Shand and Mason, under their patent dated April 11th, 1861, and supplied to the London Fire Engine Establishment in June, 1862, Messrs. Merryweather and Sons would have walked over the course. As it was, a field day was got up: Messrs. Merryweather's was first to get steam, but, unfortunately, the nut of their piston-rod worked loose, and, for a time, disabled their engine; and, later in the day, they burst a pipe, which compelled them to withdraw from further trial. There can be no doubt that Messrs. Shand and Mason's engine No. 10 worked the most comfortably, upon this trial. They were a considerable time longer getting steam; but, when it did start, it went off smoothly. Upon the other hand, Messrs. Merryweather's engine bumped in a very unpleasant manner, sometimes banging back and forward with sufficient force to break it up, and this was the cause of its failure, and would always be a source of weakness in this style of engine; for if, from any cause whatever, the main slide valve did not act, then there was very little to prevent the piston from flying to the end of its stroke with sufficient force to knock the end or cover off the cylinder.

Shand's small engine worked very well; the most noticeable thing being the priming of the boiler, and sometimes, for fifteen or twenty minutes at a time, it was impossible to tell where the water was. It is a double-cone, the water space being between the two and the fire inside; the steam cylinders are single-acting, and placed over the pumps precisely as in the first float; the crank shaft is between them, but, instead of working in a slot, is connected to the pump piston, in the same manner as in the pump of 1858; the pumps are also single-action, exactly as the first float; and, like the hand engines, the

engine had worked at 32 fires in six months, the average time at each being two to three hours.

This engine, however, gave considerable trouble from breakages; but this might be expected, the cylinders being single acting, each piston has upon its down stroke to raise the piston plunger-rods E, C, of the other set, the result being a rapid succession of cross strains upon the crank; however, from its lightness it was found so handy that another upon the same general plan was ordered, and was supplied in February of the present year, and there had not been any serious complaint of its working.

As all the steam fire engines since made in London were tried at the Crystal Palace in July, we will now proceed to review those trials.

The jury, or perhaps, more properly, the sub-committee of Class 8 in the late Exhibition, feeling that the three engines exhibited, or rather tried in Hyde Park, did not fairly represent the skill of this country, a movement was set on foot to get a fund, and offer such prizes as would induce other makers to come forward with engines.

A committee was formed, consisting of the following noblemen and gentlemen:

Chairman, his Grace the Duke of Sutherland; Members, the Right Hon. the Earl of Caithness, Lord Richard Grosvenor, J. G. Appold, Esq., J. T. Bateman, Esq., W. M. Brown, Esq., T. R. Crampton, Esq., W. M. Crossland, Esq., W. Fairbairn, Esq., T. Hawksley, Esq., J. E. McConnell, Esq., H. Maudslay, Esq., J. Mathews, Esq., J. Penn, Esq., J. Nasmyth, Esq., W. Smith, Esq., and Captain E. M. Shaw, Hon. Sec.

A sum of money was obtained, and the following prizes offered, viz., £250 for the best, and £100 for the second best engine in each class, the classes being two, those between 30 and 60 cwt., and those under 30 cwt.

The chief points to which the committee directed, or rather were to direct, their attention, in addition to the consideration of cost and weight, were those relating to the general efficiency of the machines as fire engines; combining among other points of excellence, rapidity of raising and generating steam; facility of drawing water; volume thrown; distance to which it could be projected with the

least amount of loss; simplicity, accessibility and durability of parts.

Upon the faith of this, seven makers entered their names for ten engines as follow:

Merryweather and Sons, two; Shand, Mason, and Co., two; Butt and Co. (American), two; Easton and Amos, one (American design); Nicols (American), one, "Manhattan;" Gray and Son, one, and W. Roberts, one.

As two of these engines were old acquaintances, they would be taken in the order of seniority. First:

THE TORRENT.—Merryweather, Sons, and E. Field, inventors and manufacturers, 63, Long-acre, London. (Plates No. 4, 5, 6.)

Fig. 1 is an elevation of a single cylinder steam fire engine for land service, constructed according to this invention; and Fig. 2 is a vertical longitudinal section of the steam cylinder and valves appertaining thereto. The framing of the machine *a*, *a*, is formed principally of angle iron supported on springs, and carried by the travelling wheels *b*. The boiler *c* is attached to and carried by the framing at a level below the steam and water spaces, so that the bolts employed for fixing do not pass into either of them, thus avoiding liability to leakage. It communicates by the steam pipe *d* with the valve chest *e*, as also by a small pipe d^1 with the valve chest e^1, made use of for regulating the motion of the piston valve, whereby the ingress and egress of steam to and from the main cylinder are regulated. The construction of the cylinder and valves is clearly shown at Fig. 2, in which the same letters are employed to indicate the same parts as in the other figures. The two pistons f, f^1, of a piston valve work in a suitable cylindrical valve chamber or chest *e*, into which steam is led from the steam pipe *d*. The rod f^2 of the piston valve is continued forward, and fitted with a small piston *i* working in a cylinder, to which steam is admitted alternately on each side of the piston as required by means of a small slide valve i^2 of ordinary construction. The valve chamber *e* communicates with the main cylinder *j* by means of four passages k, k^1, and l, l^1, which open into thoroughfares m, m^1, communicating respectively with the forward and backward ends of the cylinder. Small passages n, n^1, lead into the main thoroughfares m, m^1, and are formed with cocks for the

purpose of regulating the escape of steam after cushioning. The action of the valve piston is regulated by means of levers o, o^1, connected by links p, p^1, with the piston rod, common to both the steam cylinder j and pump q. The levers o, o^1, communicate motion to a rocking shaft r, upon which is keyed a disc, s, fitted with adjustable stops, which act alternately upon each side of a tongue lever, t, taking into the rod of the slide l^2. In the position of the parts as shown, the piston j^1 propelled by steam (admitted through the opening k to its forward side), has just completed its backward stroke, and by closing the backward main port has caused the steam behind the piston to form a cushion, whereby the momentum of the piston has been checked sufficiently to avoid injury to the end or cover of the cylinder; while the steam forming the cushion has, at the same time, been gradually escaping through the opening n into the exhaust passage l^1. The piston-rod during its backward action has given motion in that direction to the levers o, o^1, the shaft r, and disc s; the latter moving through a great portion of its arc without moving the tongue lever t. When, however, the corresponding adjustable stop has come into contact with the forward side of the tongue lever, the slide i^2 is immediately moved into the position shown, and steam is thereby admitted to the forward side of the small piston i, thus driving it backward, and carrying the pistons f, f^1, into the position shown by the dotted lines, and causing the piston j^1 to be moved in the contrary or forward direction; firstly, by the passage of steam through the small passage n to the back of the piston, which, upon passing, and thus laying open the back port, is continued in its forward motion by the full volume of steam entering thereby, until upon the piston j arriving near to its forward stroke, the piston i is again moved, and the contrary action takes place. It is obvious that the frame, boiler, and general arrangements herein represented and described with reference to this engine may also be applied to engines having two or more cylinders and pumps.

Fig. 3 is a vertical section of the boiler; and Fig. 4, an enlarged section of one pair of the tubes detached. The fire-grate is of the ordinary or any other suitable construction, and is enclosed in a casing v depending from the water and steam space u, u^1. From the tube plate x depend and descend into the furnace a series of tubes,

x^1, curved or otherwise, as found most convenient for the economical employment of the heat generated in the furnace. These tubes, x^1, contain other tubes, x^2, which are notched, or otherwise made so as to be freely open at bottom, and are formed with enlarged trumpet-mouths at their upper ends, or are otherwise equivalently shaped, to deflect the steam and water ascending from the annular spaces contained between the inner and outer tubes, in such manner as to facilitate and not to interfere with the downward current of colder and solid water, descending by the inner tube to replenish that which has passed upward by evaporation or otherwise between the inner and outer tubes. This deflection of the upward current may be effected by various arrangements, differing more or less from the trumpet-mouth hereinbefore described and shown; as, for example, by the use of a dish or cup-shaped top to the inner tube, or by an annular disc or washer, both of which are substantially equivalent to the trumpet-mouth.

The steam, after passing through the engine, as described, is carried off by an exhaust pipe, y, which terminates in a box or chest, y^1, having an opening opposite to the bottom of the chimney; from which opening the steam rushes upward, and increases the draught of air through the fire-grate; the lower sides or surface of the box or chest acting at the same time as a baffle plate, deflecting the flame and heated gasses, and preventing their too rapid escape up the chimney. The circumference of the tube plate x is bent downwards so as to form an annular pocket, x^3, for the collection of mud or sediment thrown up and ejected from the tubes, and which, after settling in the pocket, may be got rid of when required by openings provided for that purpose.

The pump is double-acting, of $4\frac{3}{4}$ in. diameter, with a stroke of 12 in., the piston being moved direct by the steam piston: the water passages and valves are large and easy of access, and are placed beneath the pump cylinder: there is a simple method of supplying oil to the piston, so that it lubricates itself at each stroke.

The whole is mounted on a strong-wrought iron frame, pivoted over the fore carriage, and its total weight, made up for running, with hose, coals, water, &c., is 1 ton 18 cwt.

This engine was considerably reduced in weight, to bring it within

the 30 cwt.; even the coal bunkers and foot plate were taken off, rendering it for all practical purposes useless as a steam fire engine; yet still it was over weight, being 30 cwt. 1 qr. 12 lb., but it was allowed to work in the small class. Its performance will be noticed presently.

(Plate No. 7.)—The next engine was the one made by Messrs. Easton and Amos, from designs by Messrs. Lee, and was, therefore, to all intents and purposes, an American engine. It was called the "Sabrina," but was formerly known as the "Annihilator," under which name it appeared at Lambeth. At this time its weight was 3 tons 2½ cwt., but for the trial at the Crystal Palace was cut down to 2 tons 18 cwt. 3 qr. 12 lb.

Having been referred to Mr. Hodges' book for a description of it, by Messrs. Easton and Amos, the author would again borrow from those pages, *but must state that he did not endorse all there stated.* He considered the arrangements for working the slide valves of this engine—also those of Messrs. Merryweather and Sons—too complicated; there were too many working parts, very costly to make, and would no doubt, be found very costly to keep in repair. In this style of valve motion it was absolutely necessary to arrange for cushioning, and altogether it appeared to be improving backwards, going towards the tappet motion. This opinion was strengthened by the fact that Messrs. Lee (who having perhaps as much experience in steam fire engines or quick working engines) had adopted the eccentric for working the slide valves of the two engines they imported for the competition.

The Annihilator.—Wellington Lee, of Brooklyn, New York, and London, inventor: Easton, Amos, and Sons, the Grove, Southwark, London, manufacturers.

The principal points in which this engine differs from others which have been already constructed and used in this country, comprise:

Firstly. A description of boiler of a novel and somewhat peculiar construction, having a central furnace surrounded by a shell or wall of vertical water tubes surmounted by a steam chamber or drum, which in ordinary work, is filled with water to about one-third of its

height; from this chamber depends a flat water space, or "suspended slab," the connexion being made with the steam drum by means of a series of vertical tubes. Through these last proceed a series of internal tubes, by which the products of combustion pass in an intensely heated state to the smoke box, exposing, by this means, an annular water space to the action of the heat.

A series of short tubes pass, independently of these, through the suspended slab and the steam drum respectively, through which the heated current also passes; and the entire arrangement is so adapted as to present the greatest possible amount of heating surface obtainable to the action of the fire.

A series of tubes pass from the suspended slab to the water bottom into which the bottoms of the outer shells of tubes are secured, thus maintaining the complete circulation of the water throughout the boiler.

The total amount of direct heating surface in the boiler is 228·5 square feet; and of fire-bar ditto, 4·58 square feet.

Secondly. In the construction of the boiler, gun metal and steel, in conjunction with the best Lowmoor iron, are alone employed, with a view of obtaining the two essentials of lightness and compactness, in addition to the rapid generation of steam.

Thirdly. The steam cylinders are two in number, and are placed immediately forward of the boiler; their diameter is 9½ in., the length of stroke 9½ in., and they work directly on the pumps without the intervention of any crank or rotatory movement, and, consequently, any centre or dead point to turn. The piston-rod of one cylinder, by means of a reducing lever, operates upon the slide valve of the other in such a manner that when one piston is at the end of its stroke, the other is at half stroke, and *vice versâ*. This arrangement, while ensuring the correct action of the slides, for the admission and exhaustion of steam to the cylinders, is not of itself alone sufficient to issue the proper length of stroke, but to avoid the breaking of piston or cylinder cover, which might, perhaps occur. To guard against this, two supplementary parts are provided, so arranged that the exhaust is imprisoned shortly before the termination of the stroke, and the piston starts smoothly and evenly on its

return, and, however rapid may be the running, the motion is as certain and even as in two engines working with cranks at right angles upon one shaft.

Fourthly. The pumps are two in number, each $5\frac{5}{8}$ in. diameter; but the plungers and seats may be changed in about twenty minutes for others of a larger diameter, in case a greater quantity of water may be required. The length of stroke is $9\frac{1}{2}$ in., and being double acting, a steady and continuous stream is obtained from them. Each pump has eight suction and eight delivery valves, of india-rubber, working upon gun-metal guards, offering an effective water-way of 15 square inches (in four valves), or very nearly two-thirds area of the piston for the contents of one pump.

The largeness of the valve-ways, combined with the peculiar stop at the end of each stroke, which is a main feature of the steam-valve motion, causes the almost instantaneous closing of the valves, and the pumps run free from concussion or vibration at any practicable velocity. The net area of the suction-opening is 16 square inches, and, having a continuous stream passing through it, the hose remains steady and quiet when the pumps are running at their highest velocity; moreover, advantage is taken of the hollow spaces of the hand railing to connect them with the suction-valve chamber, so as to form a suction air vessel.

All the suction and delivery valves are readily accessible by means of suitable hand-holes, and are not easily choked.

Lastly. The framing of the engine is of wrought-iron throughout, forged entire. Fisher's busk springs, as offering the greatest elasticity and lightness, are employed, with relieving screws for locking them out of gear when working. The wheels are of a construction embodying great strength and stiffness with lightness, and wrought-iron rings and dowels are used for securing the spokes to the hubs.

The engine, besides a moderate quantity of coal, and several hundred feet of hose, carries the following staff: One driver, two look-out men (one on each foot-plate), and three assistants, who ride upon the tool chest (which, when in position, forms a foot-board behind), steadying themselves by the hand-railing.

The nett weight of the engine is 3 tons $2\frac{1}{2}$ cwt.

We will now consider the engines that made their first appearance

on the 1st of July, and it will be as well to take them as they appeared in the award of the committee: First, the "Sutherland," Merryweather, Sons, and Field. The weight of the engine was 2 tons 18 cwt.; it has two steam cylinders $8\frac{5}{8}$ in. diameter, with 24-in. stroke, the two pumps being $6\frac{1}{8}$ in. diameter, also 24-in. stroke. The boiler is similar to the "Torrent's," and is described as being vertical, tubulous, circulating, shell of homogeneous metal, $\frac{5}{16}$ in. thick full, double riveted; tube and top plate of Lowmoor iron, $\frac{1}{16}$ in. thick; stays of bowling iron, 1 in. diameter; tubes of solid drawn copper, height 60 in., diameter 42 in., and will contain from 30 to 90 gallons of water (a rather wide margin) when at work.

(Plate No. 8.)—The pumps are the same description as those in the "Deluge" and the "Torrent;" but the valve arrangements must have received an immense amount of consideration. It is thus described in the *Mechanics' Magazine* of July the 10th: "On the middle of each piston-rod is keyed a boss, carrying a short arm projecting horizontally. Parallel to, and at a short distance from, each piston-rod are fixed in suitable bearings, so as to revolve freely on their axes, two twisted bars, or quick screws, having a pitch of one turn in 16 in. At the ends of these twisted bars or screws, next the steam cylinders, are cut two strong square-threaded screws, having one turn in $1\frac{1}{4}$ in., on to which are fitted two gun-metal nuts, which nuts are received by the forked ends of the weigh shaft levers for moving the slide valves. To the short arms on the piston-rods above mentioned are attached two gun-metal sliding pieces, which clasp and move freely on the twisted bars or screws, and, having the same motion as the piston-rods, impart a slow, easy, reciprocating, rotating motion to the twisted bars, causing the gun-metal nuts and weigh shaft levers to be brought backward and forward with a slow, easy motion, thus moving the slide valves into the required position, viz. that of closing steam and exhaust ports shortly before the end of the stroke, thus preventing the possibility of striking the ends of the cylinders. It will be seen that by this arrangement each cylinder cuts off its own steam and exhaust, and is entirely independent of the other for forming the cushion required to stop the momentum of the piston; thus each piston brings itself to rest; but when at half-stroke, by means of a connexion between the weigh bridge levers, gives steam

to No. 2 cylinder, the piston of which brings itself to rest, and liberates No. 1 piston, and so on alternately."

To use the words of one of our advertising watchmakers, this looks very like the perfection of mechanism, but it would be found rather too much so for rough work. The author could bear witness that it did bring the pistons to rest most effectually at the end of the stroke, and very nearly so in the middle, giving anything but the easy motion claimed for it. He also observed when it was working at Lambeth, that every time this check occurred, the water pressure fell from 10 lb. to 15 lb. per inch, an irregularity he never before saw with a double-cylinder engine.

At the trials on the 1st of July, this engine was second to get steam to 100 lb. per inch, and show water, but was first into the hood, and, except being occasionally short of steam, worked very well during the day, but nearly came to grief by cracking the end of the pumps. However, this was repaired in the course of the night, the engine being taken off the ground, which the committee ought not to have allowed on any consideration. The engines should have remained upon the ground, and if incapable of going through the whole of the trials, should have been disqualified. *There could be no objection to anything that could be done readily on the grounds*, but certainly the engines ought never to have left the grounds until the trials had ended.

On the second day at the 18 ft. lift, this engine being primed, or rather the pumps and suction pipes, fetched the water immediately, from the simple reason they had no distance to fetch it, and filled the tank in 1 hr. 24 min. 55 sec., and against the water tower threw a $1\frac{5}{8}$ jet 180 ft. high, for a few seconds, and was awarded the first prize of 250*l*.

The next engine was by Shand, Mason, and Co., and is thus described: The boiler, vertical tubular; diameter at fire box, 42 in.; diameter of barrel, 32 in.; height, 60 in.; iron and brass tubes. This was the most brief description ever met with, but there is a full description of the boiler of the small engine, which is identical in construction, but of smaller size, in the *Mechanics' Magazine*, which will be referred to presently.

(Plate No. 9.)—The boiler of the large engine, by the same firm,

is almost precisely the same, in every respect but size, as that to be described, the number of tubes being 328, 1 in. in diameter outside, and 14 in. long, divided into four groups, in order to permit free circulation. The top plate of the smoke box is strengthened by eight tubular stays $1\frac{1}{4}$ in. in diameter inside, which suspend it from the boiler, being screwed into the smoke-box plate, and stopped by suitable screwed caps at the other end outside the shell. These stays act as additional heating surface, although there is no current through them. There are no very remarkable features presented by the engines or pumps, the dimensions of which have already been given.

The weight of this engine was 2 tons 17 cwt. 12 lb., and it had two steam cylinders of $8\frac{1}{2}$ in. with 9 in. stroke, and water cylinders of 7 in. diameter, also 9 in. stroke, and appeared to be two of the engines used in No. 10 (formerly described) placed side by side.

This engine was third to get steam to 100 lb., the time being 11 min. 45 sec., instead of 10 min. 51 sec., as stated in the report; the water in this boiler was very unsteady, as might be expected from the closeness of the tubes, about $\frac{1}{4}$ in. apart.

The engine plunged a good deal when working quickly, but the jet thrown was steady.

Upon the second day's trial, there was considerable difficulty to fetch the water at the 18 ft. lift, the pumps had to be primed repeatedly, they not having a foot valve in the suction pipe, the water ran away before the rose could be got into the water, but when they did get to work they went on very steadily. At the trial against the tower for height, this engine threw a very steady stream, the jet being $1\frac{3}{8}$ in., but from some cause did not long continue working. The cause was believed to be that the committee had said that was sufficient, and Mr. Shand being anxious to get back to his small engine, had drawn his fire; but there were some gentlemen there who had taken a very active part in assisting one competitor in particular, and who wanted more show, and so some of the engines were kept to work after Mr. Shand had left. Messrs. Shand, Mason, and Co. obtained the second prize of 100*l*.

The next engine mentioned in the awards was the one the author had the honour to exhibit, but which he did not intend to work in this class.

DESCRIPTION OF W. ROBERTS'S STEAM FIRE ENGINE "PRINCESS OF WALES." (PLATES 9, 10, 11.)

Plate No. 9 is a side view of the engine; Plate No. 10 a longitudinal section of the same; Plate No. 11 is a side view of the engine pump and boiler, and a front view of the engine. The same letters refer to the same parts in all the plates.

The framing consists of light angle and strip iron, and the hose box (C, fig. A) of plate iron; this is large enough to carry 800 feet of hose, beside buckets, tools, &c. The driver's seat will carry 4 branch pipes and rests, a stand-pipe, saw, and other tools, and the tool box (M) the whole of the spanners and tools necessary to take the pump or engine to pieces, also spare gauge glasses, &c.

The seat (A A) is capable of accommodating 16 men beside the driver, and the foot plate at least 2 more. Under these seats there is room for five 6-feet lengths of suction hose and three 6-feet ladders. This is the only steam fire engine that carries ladders.

The shell of the boiler is made of treble refined iron $\frac{5}{16}$ in. thick, the tube plates $\frac{1}{2}$ in. thick. The tubes are all best lapwelded steam tubes of No. 11 wire gauge, having one end swelled and screwed into the tube plates top and bottom, so that each tube forms a stay.

The fire box is also composed of best lapwelded steam tubes, the lower ends being screwed into a tube ring, and every alternate tube going straight up into the boiler, and every other tube bending inwards, as shown, and being screwed into a place left for it among the other tubes. The action is this: the feed water goes into the tube ring at the bottom, then rising through the tubes enters the boiler, in forty places, thus ensuring perfect circulation. The steam is taken off in a similar manner.

The water tank (D) is fitted round the boiler, and is supplied from the main delivery, the quantity being regulated by means of a ball valve.

The steam cylinder is of the ordinary style, with a plain piston and slide valve. The cross head (*a a*, Plate No. 11) is connected with the levers (*c*) by means of the rods (*b b*); these levers, one of which is a bell crank, shown in the side view, are securely keyed to the shaft (*i*) running through the pump, the short arm of the bell crank lever is connected by a rod (*d*) to the fly wheel, and by means

of the shaft an eccentric motion is given to the slide valve in the usual manner.

Upon (*i*) is a cross head, each arm of which is rather less than a fourth the length of the lever (*c*), so that the stroke of the pump buckets is rather less than one-fourth of the stroke of the piston; in each end of the pump, one above and one below, are the buckets, having a fork to each, which act as guides, and are connected to the cross head by links. It will readily be seen by this that as the piston travels up and down the buckets recede and advance; thus, as the lower bucket rises it lifts the water through the upper bucket, and discharges a volume due to the diameter of the bucket and length of stroke; upon the return stroke of the engine the upper bucket rises and delivers its volume of water, and at the same time fills the lower chamber ready for the next stroke of the lower buckets.

It will be seen by this that the water is constantly flowing through the suction pipes and pump in one unbroken line, giving as much as 20 per cent. over the theoretical quantity due to the pump when working with open delivery, and there is very seldom any loss under 25 to 30 lbs. per inch. Each bucket is a grating, having a disc valve of large dial and small lift, and the buckets, valves, forks, and links are duplicates of each other. The upper bucket is readily got at by taking off the air chamber, and the lower one by taking off the cap at bottom.

Every part of both engine and pump are easily accessible in case of need, neither the cylinder cover or steam jacket being covered.

The engine was first to get steam to 100 lb. and throw water, but from its position, being placed to windward of all the others, and its small size, it was of course beaten in quantity, but not in steady working.

At the 18 ft. lift it fetched the water immediately without priming, although a piece of canvas near half a yard square was afterwards found in the pump, and in the first hour's working it threw exactly the same quantity of water into the water temple that Shand and Co.'s large engine did, viz. 20 in.; but shortly after this the head of a rivet came off, and the engine was worked easy for fear of breaking down altogether. At the trial at the tower (this rivet still being out of the stay) a jet was thrown quite as high as by any engine, the jet being $\frac{7}{8}$ in.

Throughout the whole of the trials this engine worked steadily, there was scarcely any oscillation, no priming, very little variation in either steam or water pressure, and, taking power into consideration, contrasts very favourably with the prize engines; thus, in the first trial the "Sutherland," with steam cylinders more than *five and a half times* the capacity, and water cylinders *six and three-quarter times* the capacity of the author's pump, it took 9 min. 42 sec. to do the work that it took 20 min. 24 sec. to do with his. The steam pressure at starting being the same, viz. 100 lb. per inch, or, in other words, two and one-quarter times the work was done with five and a half times the power. Again, in Shand, Mason, and Co.'s engine, the steam cylinders were more than twice the capacity of his, and the water cylinders three and a quarter times, and yet it took them 12 min. 19 sec. to fill the tank, or one-fourth longer than he should do it in, according to the time taken by his engine, and this, too, when, from its position, it had the benefit of all the spray from the other engines.

Again, take the third trial, which was, in fact, the principal trial. Merryweather's engine delivered 16,086 gallons in 1 h. 24 min. 55 sec., or say 189 gallons per minute, with an average steam pressure of 91 lb., and water pressure of 89 lb.; and the author's engine delivered in the first hour before the rivet gave way, 5560 gallons, or 92 per minute, just half its more bulky competitor, and this with an average pressure of 75 lb. both steam and water, and if the whole two hours are taken it brings it exactly the same as the first trial, viz. two and a quarter times the work for five and a half times the power; and as Shand's engine only delivered the same quantity in the first hour and not one-third more in the two hours, the author thought he had said enough to prove that the best engine did not obtain either the first or second prize, and if the trial at the water tower had been taken the case would not be altered.

Having previously described the "Annihilator," *alias* the "Sabrina," it is only necessary to say that it was fourth to get steam, and did very good work at the low lift, but could not accomplish the 18 ft. lift, and was finally withdrawn; the most noticeable point being that during the time it did work the average water pressure was only 41 lb. to 98 lb. steam.

The next was an engine called the "Victoria," built by the Amoskeag Company, of America, and imported by Messrs. Lee, but entered under the name of Butt and Co. This engine had a very light appearance, having just sufficient framing to carry the boiler and engine upon wheels; there was a seat for the driver and room on the foot-plate for two or three men, but no hose-box. The fore carriage would not lock to above 35 to 40 in.—a very serious objection to an engine intended for a place like London, with narrow streets. This engine had a vertical tubular boiler, with submerged smoke-box, made of iron and steel; diameter, 36¾ in.; height, 65 in.; height of fire-box, 20 in.; number of tubes, 313; diameter outside, 1¼ in.; distance between sheets of submerged smoke-box, 4½ in.; length of submerged smoke-pipe, 16 in. The arrangement of the engine is similar to Shand and Co.'s No. 10, except that it is vertical instead of horizontal. The steam cylinder is 10½ in. diameter, and the pumps 6 in., with a stroke of 12 in. The crank shaft is in a line with the centre of the frame, and the fly-wheel work across; this causes a very large amount of oscillation, and would be found very destructive, in consequence of the very rapid working of the machine. The engine was 17 min. raising steam to 100 lb., but when it did get to work it made up for it by filling the tank in 6 min. 48 sec. Upon the third trial this engine fetched the water immediately, but after working some little time got uneasy and fitful, and finally knocked the cylinder cover off. This was hastily, but not effectually, repaired in the night, and it would be unfair to refer to its performance at the water tower.

There was one other engine of which great expectations were formed—the "Manhattan," No. 8. This was sent over by the New York Fire Company, and was the crack engine there. This engine had the same fault in the locking carriage as the last, and in going to the place of trial capsized, seriously injuring one man. It was tried upon the second day, and considered fit for work by those in charge. It certainly threw a very good jet, but upon trying it the next day at the high lift and long range it soon became evident that it was unable to do it, and finally came to grief by bursting the boss of the remaining fly-wheel. This engine was repaired, and afterwards tried at the Shadwell entrance of the London Docks, but totally failed to maintain its reputation.

There was one other engine entered for this class, one made by Gray and Sons, of Limehouse. This had a rotating boiler, but was not got to work.

"Small class engines." Strictly speaking, there were none under 30 cwt. The "Torrent," as before observed, had its coal lockers and foot-plate taken off, besides other alterations. Shand, Mason, and Co.'s engine had no hose reel, although fitted for one; and Messrs. Lee's had some small parings to bring it to weight. The steam cylinder of Shand, Mason, and Co's engine was 7 in., with an 8-in stroke; the pump was of the combined bucket and plunger, the plunger being hollow. The crank shaft was placed fore and aft, and the fly-wheel athwartships, the connecting working in the hollow plunger. In the report the diameter of water cylinder is given as 9 in., but the diameter of plunger is not stated. The bucket has six and the foot-plate seven india-rubber disc valves, $2\frac{3}{4}$ in diameter. The boiler is thus described in the *Mechanics' Magazine*, July 10, page 448:

"The upper part of the boiler is cylindrical, of Bowling 'best best' iron, one-fourth of an inch thick, welded, 2 ft. 1 in. in diameter outside, and about 2 ft. 6 in. high. The upper part, turned to a round flange, is bolted to a suitable plate, through which the up-take 8 in. in diameter passes. The lower part has a Bowling $2\frac{1}{2}$ in. angle iron ring riveted round it. The fire box shell is conical, 2 ft. 1 in. in diameter at the small end, which is fitted with a precisely similar angle-iron ring, and 2 ft. 10 in. in diameter at the larger, outside, giving a diameter of circular grate of 2 ft. $7\frac{1}{2}$ in. Both rings are accurately turned and faced in a lathe, so as to make a perfectly steam-tight joint, by the aid of a little red-lead, when bolted together. The inside fire box is of iron of the same thickness, the top, which forms the tube plate, excepted, this last being also of iron, but $\frac{1}{2}$ in. thick. The vertical tubes are 195 in number, 1 in. in diameter outside, 12 in. long, and No. 19 wire-gauge thick, fitted without ferrules by simply drifting. They are spaced only one-fourth of an inch asunder, and deliver the products of combustion into a smoke box about 5 in. deep, quite covered with water. From the centre of this smoke-box rises the up-take, surmounted by a handsome brass-topped stack, 2 ft. 9 in. high. The distance from the grate bars to the tube

plate is 2 ft., the height of the boiler over all about 4 ft. 6 in., the bottom clearing the ground about 15 in. By unscrewing the bolts which connect the angle irons, and those which unite the upper flange with the top plate, the entire cylinder shell can be removed, permitting access to every portion of the interior of the boiler tubes, smoke box, &c., an advantage of the last importance. A space of some 4 in., however, unavoidably remains between the boiler shell and the outer tubes, because the shell could not be drawn over the smoke box if its diameter were narrowed in order to approach the tubes. An unnecessary body of water would, of course, remain here, but for the following ingenious expedient: A species of copper 'pocket'—we cannot think of a better name—descends nearly to the crown of the surface all round and between the shell and the outer tubes, displacing the water, and supplying so much additional steam space, for the upper edges of this pocket are considerably above the water line—the boiler, in fact, carries steam below the water. Of course no pressure except that due to the gravity of the liquid, is exerted on the sides of the pocket, which are traversed by the pipes leading to the gauges. The contrivance is extremely simple, and answers its purpose admirably. A copper ring, drilled with a multitude of holes, surrounds the up-take, and supplies the steam to the cylinder through a copper tube 1½ in. diameter, fitted with expansion joints. The exhaust steam pipe is 2½ in. diameter, terminating in a variable exhaust, not easily described without drawings; suffice it to say that the orifice can be enlarged or contracted by means of a hand-screw, being equivalent in area to a circle 1¼ in. in diameter when full open, and to one of ¾ in. when shut.

"The fire box is provided with two fire doors—one behind, and the other at an angle of 45 deg.—just in front of the hind wheel. A person standing on the foot plate behind can fire the boiler when proceeding through the streets. The 24 ft. of suction which the machine carries ready screwed on, being coiled round the engine out of the way. On reaching the fire this door is shut, and the front wheels locked round; the other door then becomes easily accessible, a small coal bunker being hung from the front axle, in a convenient position for the stoker. The weight of this engine as entered for the trial was 1 ton 9½ cwt."

EXPLANATION OF VERTICAL SECTION OF MESSRS. SHAND AND MASON'S PATENT STEAM FIRE ENGINE. (PLATE, NO. 12.)

The boiler is constructed with a conical fire-box, A, vertical tubes, C, and a submerged smoke-box, B. The steam-jackets, D, communicate with the steam space in the upper part of the boiler by the open pipes, E, and they have small cocks at the bottom to draw off any water that may find its way in. The boiler can be taken apart for repairs by the bolted joints at F, G, and H.

The engine is made with an inverted steam cylinder, I, placed above the pump, K, which are framed together by the four bars, L, that also carry the crank bearings. The pump is fitted with a hollow plunger, N, and bucket, O; at the bottom of the barrel is the grating, P, which, as well as the bucket, is fitted with india-rubber valves. The head of the pump, K, is fitted with an air vessel, Q, and nozzles to take the hose at R; over the openings at S is fitted a valve, shown in plan at Fig. 3, which is so made that it is impossible to shut off both hose at once, which might burst the air vessel. The connecting rod, T, is jointed to the plunger, which is attached to the steam piston by two piston rods, between which the crank works, as shown in Fig. 2. Upon the end of the crank shaft is an eccentric, which works the slide and the feed pump, V.

Fig. 4 is a section of the governor, which is composed of a small cylinder, the piston of which is connected to the steam valve: the pipe, W, leads to the steam chest, and the pipe, X, to the pump head; any change of pressure in the pump will allow the piston to be moved by the steam, and so regulate the speed of the engine.

This engine got up steam to 100 lb. in 11m. 36s., and filled the tank in 5m. 24s.; on the third trial steam was got up about ten o'clock, but the engine could not be got to work, first some shaving got into the pump and stopped, or rather prevented its working, then something else went wrong, and finally it was got ready to start at 7 p.m., yet in the report the remark is "worked well throughout;" the report also states that the average pressure of steam was 146 lb., and the water 80lb., but certainly when the author saw it at work the water did not exceed 60 lb. Now there must have been something very wrong here, 146 lb. of steam and 60 lb. only of water. His engine with the same size

jet, the same length of hose, the same lift, in all things similar (except the wind, which was blowing strong when his engine was tried, and a dead calm when Shand's was tried), with 75 lb. of steam gave 75 lb. water. Lee and Co.'s "Victoria," with 78 lb. steam, gave 78 lb. water; the "Sutherland," with 91 lb. of steam gave 89 lb. water (again nearly equal); Shand's large engine, 96 lb. steam, 62 lb. water (one-third less); Easton and Amos, 98 lb. steam to 41 lb. water; this was worst of all; the "Torrent," 86 lb. steam, and 45 lb. water; and the "Alexandria," 80 lb. steam, 60 lb. water.

During the trial the fire was from one to two feet above the top of the chimney, a state of things no boiler with tubes made of 19 guage, and spaced a fourth of an inch apart, could long stand. The oscillation of this engine was also fearful.

The "Alexandra," also made by the Amoskeag Company, and imported by Messrs. Lee, was in every way similar to the "Victoria," but smaller, having a $7\frac{3}{4}$ in. steam, and $4\frac{1}{4}$ in. water cylinder, with a $9\frac{1}{2}$ in. stroke; this engine got steam in 11m. 55s. and filled the tank in 6m. 3s., and at the long lift and range fetched the water directly, and worked on its allotted hour without stop, and obtained the second prize of 100*l*.

Having fully described the "Torrent," it is only necessary to say that it got steam to 100 lb. per inch in 12m. 15s. and filled the tank in 9m. 15s.; at the third trial very great difficulty was found to fetch the water—in fact it was not until it had been repeatedly primed that it succeeded, but after it did get to work it went on very steadily.

With regard to the committee, the author believed every member was actuated by the best motives, and thought they deserved the best thanks of society for the trouble they took in this matter; but they were unfortunately in the position of the man who tried to please everybody, and so pleased nobody; had they stuck to their rules, and carried them out to the letter, they would be just as well thought of.

In bringing this matter before the Society, the author had carefully avoided anything like imputing motives, or of personalities, and he hoped that all who might take part in the discussion would bear in mind that personalities were not arguments; but he could not con-

clude without expressing his belief, that had the engines been subjected to the whole of the tests, a very different result would have been obtained; some of the engines were mere racers, never intended for, or at least capable of, continuous hard work; some would carry scarcely any hose or gear—all points that should have been strictly inquired into, but which appeared to have been lost sight of.

DISCUSSION.

The CHAIRMAN was sure the members would readily accord their thanks to Mr. Roberts for the carefully considered paper he had just submitted. The reading of the paper having occupied a longer time than usual, the period allotted to discussion had been reduced, but at the same time it was desirable that the discussion should be opened, and so continue it at the next meeting of the society, and he hoped the members would come to the next meeting prepared to join in the full discussion of the question.

Mr. J. D. HUMPHREYS begged to correct some of the remarks made by the author in his paper concerning Messrs. Shand and Mason's small steam fire engine. In the first place, as to the thin iron tubes spoken of as being likely to be soon destroyed, he would state that there were no iron tubes in the boiler, but that the tubes were made of brass, as was distinctly stated in the report of the committee. The fact of there being no hose reel did not render this engine more incomplete than the other engines, as none had them. The deductions the author drew from the great disparity between the steam and water pressures were erroneous, as the steam gauge was not fixed upon the steam cylinder, but upon the boiler, thus showing that the steam was bottled up, for, the foot valve having been strained in the morning, in snatching for water when the shavings were in the pump, and the inner coating of the suction pipe collapsed, the engine was worked as gently as possible, knowing from the gauge, minute by minute, that she was doing double the work of either of her antagonists.

Mr. OLRICK said, before entering upon any detailed remarks, he wished to make a few general observations that applied to all steam fire engines. In the first place there was no doubt that all the English engines beat the American in getting steam up quickly, and this

fact is entirely owing to the fact that the English engines are provided with a steam jet in the chimney, which neither of the American engines have. It had been urged that this was a point of less importance, but he (Mr. Olrick) considered it of great importance, as often five or ten minutes difference in commencing to pour water upon a fire will have the effect, that valuable property, otherwise lost, would be saved. Next he had heard the injector objected to, indeed he had seen it condemned himself, but simply for want of proper management, because when he himself set it to work, it worked very satisfactorily. Therefore the question was simply as to the manner in which it was worked. The injector was not more liable to derangement than any other pump. The next point was the trial of lifting the water from a reservoir 18 ft. below the pump, which he considered of great practical importance. But there was another trial, which was entirely omitted in the Crystal Palace, viz. the test of ascertaining the actual delivery, in a tank close by the engine, compared with the theoretical. The "Torrent," for instance, he had seen at a former trial for quantity, deliver more water than it ought to theoretically, which could only be explained by the momentum of the water forcing water through the valves, when the pump piston was at its two dead centres. Many objected to the trial of throwing water into a hood elevated to a certain height, and did not consider it a good test, he (Mr. Olrick) did, because if a fire commenced at the top of a very high house it was not of the slightest use for an engine only to be able to pump a quantity of water into the cellars, therefore, if a fire commenced high up, it certainly was a very good test of the quality of an engine if it delivered water upon the spot on fire. As regarded the wind affecting the delivery of the stream, he certainly was of opinion that each engine ought to have its turn of exposure to the action of the wind. Before proceeding to make any detailed remarks as to the trials at the Crystal Palace, it was right, he thought, to say that members of a society like this, when discussing such questions as that now before the meeting, ought to come forward and speak their minds freely without fear of being looked upon unpleasantly by those whose work they were obliged to criticise. In the first place, he had to complain of the official printed report, because it contained several inaccuracies. For

instance, with respect to raising the steam, it stated that Messrs. Merryweather and Sons, and Messrs. Shand and Mason's raised steam in respectively 10 min. 25 sec. and 10 min. 51 sec., and Mr. Roberts's in 11 min. 40 sec., whereas it in reality was respectively in 10 min. 15 sec., 11 min. 45 sec., and 10 min. 17 sec. It had also been stated in the official report that Messrs. Easton, Amos, and Sons filled their tank first in 19 min. 30 sec. Now he (Mr. Olrick) had an excellent opportunity of noticing what happened, and he saw that the Messrs. Merryweather filled their tank first, at least a quarter of a minute before any of the other tanks were filled. With reference to the next trial of filling the large tank, able to contain 16,086 gallons, in two hours, which trial was only completed by Messrs. Merryweather and Son's engine, "Sutherland," in 1 hr. 24 min. 55 sec.—it was certainly true that Messrs. Shand's engine worked at this trial under very unfavourable circumstances, as the wind blew very hard. But in comparing it with Messrs. Merryweather and Son's engine, "Sutherland," he (Mr. Olrick) had allowed, on account of the wind, 20 per cent. in favour of Messrs. Shand's, but even then the result of the trial gave 50 per cent in favour of Messrs. Merryweather. This latter engine had also this great advantage, that it could be worked at any speed to adapt itself to the quantity of water present. If this was not the case, such large engines would in many instances be useless, where the supply of water was rather scanty. A feature worth noticing in the "Sutherland" was, that however quickly it worked, it was always extremely steady. This was very unfavourably contrasted by Messrs. Butt and Co.'s engine, that shook frightfully during its working, and at last broke to pieces simply on account of that shaking. Otherwise its performance was highly satisfactory. It had been said that the "Sutherland" prize engine was primed, or more plainly that it was charged with water in the suction hose. That was not correct as he (Mr. Olrick) was present at the time. In respect to the trials for height at the tower, the official report gives the height of stream thrown by the engines of Messrs. Merryweather and Son's, and Messrs. Shand and Mason's, as 180 ft. without any remarks, but it ought to have been remarked that the "Sutherland" maintained that height for upwards of 10 min., and then a stream of 160 ft. for about 25 min., in fact, the "Sutherland," after having changed

nozzle, never ceased to work until the trial was over, whereas the others stopped repeatedly. It ought likewise to have been remarked that Messrs. Shand and Mason's engine only gave one spurt of 180 ft. and then stopped altogether. Several remarks might be made as to what was stated in the official report about the trial of the small engines, when filling the 1000 gallon tank. It states, for instance, that Messrs. Shand and Mason's engine, upon the third trial, threw nearly double the quantity of water of either of the other engines. Now, how was it possible the measurement could be correct? At any rate, upon the second trial, there was only one second difference in filling the tank, between the engines of Messrs. Shand and Mason and Messrs. Lee and Co. As regards the report of the trials which appeared in the *Times*, that certainly was a great curiosity. In describing the "Sutherland," the reporter of the *Times* states "*that the upper part of the boiler is fixed with wrought iron valves to carry off the smoke and to create a draught.*" Of course no engineer would be misled by such a statement as this, but it showed what little dependence could be placed upon newspaper reports. The reporter of the *Times* employed to describe such things as these ought to be certainly an engineer. Captain Shaw, in his report in 1862, stated that the insurance offices would save 1000*l.* by each steam fire engine, but instead of that it had cost 3500*l.* more than the average expenses for the four steamers, and instead of a saving of men which was anticipated, they have 34 men more now than formerly. He thought it would be very interesting to know the cost of all the breakdowns of the brigade steam fire engines in London. Messrs. Merryweather produced an engine that would throw 700 gallons 190 ft. for 790*l.*, while Messrs. Shand and Mason charged 800*l.* for an engine that would throw only 550 gallons 160 ft. high. For 400*l.* Messrs. Merryweather would produce an engine that would throw 250 gallons 150 ft. high, while for the same price Messrs. Shand and Mason produced an engine that would throw only 180 gallons 140 ft. high. He (Mr. Olrick) thought when they were speaking of the relative merits of an engine they should also consider whether one cost 50 per cent. more than another. As to the trials at the Crystal Palace, he had no doubt the Committee did everything in their power to make it a fair tria but at the same

time he could only say from experience that he knew what trial trips were; he had often tried and succeeded in making a vessel run much faster during a short trip than really could be accomplished during an ordinary journey. And with respect to these trials at the Crystal Palace, there were several engines he saw brought on to the ground which could be compared to nothing but racehorses, against ordinary horses. He could only say that the "Torrent," which had been hard at work for ten months, could not be expected to compete with an engine that had been made for a specific object. The "Torrent" had been present at all fires for some considerable period that had occurred within reach of Messrs. Hodge's Distillery, consequently, an engine that had done such a large amount of work, could not fairly compete with an engine made for a special purpose, with for instance tubes of only 19 Birmingham wire gauge or ·042 in. in thickness and $\frac{1}{4}$ in. apart, whereas the tubes in the "Torrent" are of 13 B.W. or .095 in. in thickness, and more than $\frac{1}{2}$ in. apart. Such thin tubes will of course give excellent results, while new and clean, but how long will they last. There is no credit due to the engine that under such circumstances can beat the "Torrent," and it was no reflection upon the "Torrent" that it was beaten by a racehorse. He thought they should not, as engineers, look upon such trials as decisive, but merely as exhibitions.

November 2nd, 1863.

R. M. CHRISTIE IN THE CHAIR.

STEAM FIRE ENGINES, AND THE LATE TRIALS AT THE CRYSTAL PALACE.

BY W. ROBERTS.

ADJOURNED DISCUSSION.

Mr. OLRICK said that since the last meeting he had had an opportunity of reading Mr. Roberts's paper. He found that it contained a great deal of valuable information, but, at the same time, there were

many points in it with which he (Mr. Olrick) could not agree. There were omitted, too, several points which it might have been expected would have been referred to, such, for instance, as the points which are indispensable in a good steam fire engine, and also the points which should be avoided. Now, Mr. Roberts had referred to the "Sutherland" in a way with which he (Mr. Olrick) did not agree. He believed the "Sutherland" was looked upon by all engineers as the best fire engine produced. To prove that the "Sutherland" was the best engine, he mentioned the facts—first, that it was the only engine that completed the very severe trial of raising water from a basin 18 feet below the engine, and in filling a tank containing 16,000 gallons, not in 2 hours, as stipulated, but in 1h. 24m., and that without being primed. Secondly, it was the only engine which, during the trials at the tower, threw the water 180 ft. high continuously for at least 10 min., and continued to throw a stream 160 ft. high for more than 25 min.; this was not done to make a show merely, because it never ceased until after the trials were over. He had no hesitation in saying that Mr. Roberts's engine showed great power, for it threw a stream 160 ft. very steadily for a long time. He was quite willing to give Mr. Roberts's engine every credit as being one of the best on the ground; but not "*the best.*" As regards Mr. Roberts's remarks about the "Sutherland" being taken off the ground for repair, he (Mr. Olrick) did not see any harm in that, because, "one stitch in time saved nine," and if fighting men could be patched up and brought up within the time, it was considered quite fair, and so it was with the "Sutherland." Mr. Roberts made a remark to the effect that on account of the "Sutherland" having been taken off the ground it should have been disqualified. Now he (Mr. Olrick) considered that would have been too severe, the more especially as the engine had completed its trial at eleven o'clock in the morning, and was then repaired during the night. It was true that Messrs. Shand and Mason's small engine did not leave the ground, but the case was altogether different, for it started the first time for the third trial at 10 o'clock, and although primed repeatedly, could not draw the water, and at last was withdrawn at 11 o'clock to be repaired, and did not work until 7 in the evening. In the morning it could not draw the water from 18 ft. below the pump,

although it was repeatedly primed. In the evening it was said to draw the water at once without priming—rather improbable—and he (Mr. Olrick) would have preferred to have seen it himself, as very few were present when the trial commenced, on account of the trials at the tower not having been finished, when this began. If any engine should have been disqualified it was that, because it could not come up to time when called upon. He was quite sure everybody would agree with him in thinking that Mr. Roberts's engine worked under very unfavourable circumstances, for it was too large for the small class, and too small for the large class; but who could help that but Mr. Roberts? as the engine was designed by Mr. Roberts especially for the trial, and he (Mr. Olrick) was willing to give every credit to the engine; but it could not come up to the large class, and did not work with the small class. He (Mr. Olrick) thought it right to examine which was the most complicated, and he could not compliment Mr. Roberts upon his being less complicated in working the slide than the "Torrent." In the latter there were only four pieces to drive the slide rod; but in Mr. Roberts's engine there were nine pieces, consequently the "Torrent" was not so complicated as Mr. Roberts's engine. The eccentric rod was so short that it gave a very bad slide motion, and he (Mr. Olrick) believed that arrangement would ultimately knock itself to pieces. Consequently, it would be a costly affair to keep in repair. In answer to Mr. Roberts's remarks that his engine did proportionally more than the "Sutherland," he (Mr. Olrick) would apply the test of the gallons of water delivered per minute per square foot of heating surface, and they would find that upon the first trial the "Sutherland" delivered ·49, and Roberts's engine ·34; upon the second trial the "Sutherland" delivered 1·55, and Mr. Roberts's did not fill the tank; and upon the third trial the "Sutherland" delivered 1·04, and Mr. Roberts's ·64 gallons per square foot of heating surface. If the test were applied of gallons delivered per pound weight of engine, the "Sutherland" also gave better results than Mr. Roberts's engine, viz. upon the first trial, "Sutherland" ·0158, Mr. Roberts's engine ·0111; second trial ·049, Mr. Roberts's did not fill the tank, and upon the third trial "Sutherland" ·033 against ·020 gallons delivered per lb. weight of engine. Query: "Did or did not the best engine get

the 250*l.*?" Of all the small engines upon the ground he should have selected the "Torrent," and for this reason, that it has a horizontal cylinder and pump, and is altogether less complicated than the prize engine; and the "Torrent," and engines like it, can be driven at any rate of velocity, and consequently can accommodate itself to the quantity of water present, which is not the case with the crank engines; and the boiler (one of Field's) requires only between two and three square feet of heating surface for one horse power, whereas in common boilers there are ten; in addition to this the violent circulation of the water prevents any sediment collecting in the tubes. Another noticeable point was the pump. The "Torrent" has a horizontal pump, the advantage of which is, that no foreign matter could remain in the cylinder, and no water was allowed to stop in the valve-box, which was a very important point in cold climates, because the water could not freeze. He believed that when the small prize engine was stopped at the trial, the pump was choked with sand. At the last meeting, he referred to the relative cost of the engines, which he considered to be of the greatest consequence when the difference was as much as 50 per cent. As regards the accident to the "Manhattan," the American papers stated that it was not an accident, but a design set on foot by Lord Palmerston. This was, of course, absurd, but although it was not the fault of Englishmen, it was the fault of one Englishman, because the engine required certainly more than five men to get it down the hill. In conclusion, he would call attention to a few points indispensable in a good steam fire engine, the cylinder and pump should be horizontal. A most important point was long-stroke pumps and a moderate velocity. There should be wide and straight water passages, and no sudden enlargements, and there should also be good large air vessels and steam jets to get up steam quickly. The points to be avoided in steam fire engines, were, first, the contraction of the water in entering the suction, from this into the pump, and thence into the hose. Secondly, bends, twists, or sudden changes of form. Thirdly, the meeting of two separate currents, as by the delivery of two alternate streams into a common passage from the two pumps. Fourthly, sudden enlargement of the water passages and all unnecessary friction, as the mere roughness at high velocities has been greatly overlooked. The points

requiring determination in order to judge of the efficiency of steam fire engines as hydraulic machines, are the following: 1. The extreme vertical height the water could be thrown. 2. The volume delivered in a unit of time *to that height.* 3. The total power given out by the engine, and consumed in performing that work in the unit of time. Having these elements we can determine the relation between the work wasted and that consumed in useful effort, and the ratio of these is the true measure of the merit of the fire engine as a hydraulic machine. In any future trial the only thing required would be to have a sort of tower in such a position as to screen the stream entirely from the wind, and where the quantity of water and height of stream could be accurately measured, and every engine should be made to use the same branch pipe, nozzle, and hose. At the recent trials at the Crystal Palace, the mean velocity of the piston was not recorded in a single instance, and this point is certainly the starting point for ascertaining the amount of useful and useless power expanded. And he (Mr. Olrick) thought that at any future trials the judges would do well to take notice of these points, as they would show beyond doubt, which fire engine was really the best hydraulic machine, without which it could not be a good steam fire engine.

Mr. F. Young said, there could be little reason to doubt the superior economy and advantages of a well-made steam fire engine. From what he had seen of these machines during the last three years, having been present at the greater part of the trials of them, he was convinced that a slow, steady, reciprocating motion is the right thing, and far superior to any fly-wheel or crank arrangement. The slow speed of the pumps is important, as it gives a full pump, and is especially valuable in using (as frequently happens) gritty or sandy water, and the higher the speed at which the engine is run, the more necessary does the slow motion become. Messrs. Shand and Mason's engine, when tried for height at the tower of the Crystal Palace, ran so hot in the pumps from the speed at which it worked, that it had to be stopped, and cold water bucketed over the pumps. He preferred the horizontal arrangement of the pumps and steam cylinders to the vertical, as conducive to greater steadiness, and better in all respects. He thought Mr. Field's boiler best in all respects; and, next, that of Mr. Roberts. Much had been debated about the non-importance of

getting up steam quickly in steam fire engines; he considered it was of the greatest importance to have the power of being ready to work in the shortest possible time, and of keeping the working pressure steadily, as the sooner a fire could be taken in hand, the more easily it could be controlled. He had carefully noted the time occupied by the several engines in getting up steam, which was:

Messrs. Merryweather and Sons . .	10 min.	15 sec.
Mr. Roberts's	10 "	17 "
Messrs. Shand and Mason's . . .	11 "	45 "
"The Sabrina"	14 "	0 "
Mr. Lee's	17 "	0 "

The author of the paper had described the steam float on the Thames. He (Mr. Young) had examined it, and seen it at work, and thought that the complicated machine was utterly disproportioned to the work done by it, and that the "Sutherland," lately purchased by the government, would do almost as much. It had been remarked that, "in the trials in Hyde Park in 1862, the 'Deluge' bumped in an unpleasant manner, bumping backward and forward, and this was stated to be the cause of its failure." He had noticed the causes of the two accidents. In the first, the nut of the piston-rod worked loose and bumped; this was soon remedied by removing the cover and screwing it up; the other was the splitting at the seam of the brazed copper-pipe to which the discharge hose was attached, an occurrence not unfrequent in the brazed copper feed pipes of locomotives. This engine had steam of 100 lb. from cold water, and started to work in 11 min. 50 sec., filling the tank in 2 min. 15 sec. the first time, and 2 min. 50 sec. the second time, the wind blowing very fresh; Shand and Mason's engine filled theirs in 3 min. 15 sec. the first time, and 3 min. the second, *their* tank being to leeward. The author objected to the method employed for working the slides in the engines of Messrs. Merryweather, and the "Sabrina" of Mr. Lee, and stated that Mr. Lee used the eccentric in the two engines he brought to the Crystal Palace trial. He (Mr. Young) thought, that where there is a rotary motion in an engine, the eccentric is as simple and good a plan for working the slide as any; and he did not see how a more simple one could be devised than that used by

those gentlemen, and he did not see that the author had suggested any other. The American engines brought over for the Crystal Palace trials were some of the finest pieces of workmanship he had ever seen; he would say nothing about their design, as he believed they had been constructed to meet the requirements of their users. So far as be knew, the American engines are of two descriptions; rotary pump and rotating slotted cross head; what these engines can do has been shown by the working of Mr. Lee's small engine that was in the Exhibition, and the "Alexandra," at the Crystal Palace. Mr. Young then referred to the late trials at the Crystal Palace, remarking that he had had difficulty in obtaining information as to the order of the arrangements, that the order was frequently not adhered to, and thought that more assistance was permitted to some engines than was consistent with strict adherence to the rules, which limited the number to be with each engine.

Mr. J. D. Humphreys did not consider Mr. Roberts's comparison between Merryweather's beautiful engine and his own to be a fair one; for who would say that the donkey engine in some tug-boat was a superior hydraulic machine to a Cornish engine, merely because through fast running the steam cylinder was smaller in proportion to the work done; but there are comparisons that may be drawn, such as the proportion of the steam cylinder to the pump, which gives nearly the duty of the engine, as the one is the power expended, and the other the work done; and he found that Messrs. Merryweather and Son's had 2.802 cubic inches of steam cylinder to 1410 inches of water cylinder, or two to one; Mr. Roberts's, 500 inches steam to 221 inches water, being more than two to one; and Messrs. Shand and Mason's, 307 inches steam to 254 inches water, being only one-fifth more; again, in the hour's work, Messrs. Shand and Mason's small engine, weighing only 1 ton 9 cwt. 2 qrs., and having 62 ft. of heating surface, delivered 8142 gallons; Mr. Roberts's engine, weighing 1 ton 19 cwt. 1 qr., with 141 ft. of heating surface, that is, weighing half a ton more, and having double the boiler power, yet only delivered 5500 gallons to Messrs. Shand and Mason's 8142 gallons; and Messrs. Merryweather's engine, weighing 2 tons 18 cwt., and having 207 ft. of heating surface, being double the weight, four times the heating surface, and nine times the

cylinder capacity, yet only delivered half as much again more water, having pumped 12,800 gallons to 8142 thrown by Messrs. Shand and Mason's small engine; now, this is a simple calculation from the published report, which any one could make, but no one can deny. As to her being a mere racehorse, if the weight be taken in comparison with the cylinder and boiler power, she was more substantial than either of her competitors, for she had only 307 inches of cylinder and 62 feet of heating surface; while Messrs. Merryweather's engine, of about the same weight, had 397 inches of cylinder and 64 feet of heating surface; and the American, 446 inches of cylinder to 106 feet of heating surface, nearly one and a half times the steam power to the same weight; but in Messrs. Shand and Mason's engine the surface was more effective and the duty higher. He considered every working part to be fairly up to its strength, and Messrs. Shand and Mason were now making engines to the same drawings. Mr. Roberts says Messrs. Shand and Mason's first engine with the crank and rods in the water worked badly; but with such experience before him, he was surprised to find him now using the same pump, with the exception that he had the levers and slings instead of the crank and rods in the water. The main points that enabled Messrs. Shand and Mason's engine to do such duty, were in the boiler, the conical fire-box giving a large fire with inclined instead of vertical surface, the small and short tubes combined with the internal pockets, replacing a mass of water by steam room. The engine was direct acting, no loss by transmitting the power through shafts, as in Mr. Roberts's, and the American with the rotary pump, and having a crank motion, it ran with certainty close to the covers at every stroke; the pump had a constant delivery in one direction, avoiding waste of power in changing the motion of the water at each stroke, as in ordinary double-acting pumps, and it was as large as the piston could drive, so as to give out its full power at a moderate speed.

Mr. D. Siebe said, it would be soon found out in practice which steam fire engine was the best, and which would last the longest. At the trial he observed one very good feature in the "Sutherland," that there was the least oscillating movement, but did not think Exhibition trials were much to be depended upon, as an engine or

machine might show very good results for a few hours, but in practical use prove inferior to others. As regards rotary pumps, he could not say much in their favour, unless all the working parts were compensating for each other. He believed the rotary pump on the "Manhattan" was the invention of his father (A. Siebe) about forty years ago, even to the screw gland which was included in his patent. This same rotary pump was tried as a steam engine, but it was found that the difference of expansion caused such a large amount of leakage of steam as to make it practically useless. Water pumps with a long, slow, and steady stroke were far less likely to get out of order, and as durability is one of the primary objects to be attended to in a steam fire engine, such pumps deserve great consideration. One great feature in Field's boilers was the great facility with which they could be repaired; for a tube could be taken out and replaced in about half an hour, whereas a Cornish boiler takes some weeks even to repair. Also, the circulation being so complete, all deposit was thrown into the mud pocket, which was not exposed to the fire.

Mr. Merryweather stated that the pumps of the "Sutherland" at the trial were not primed, in which fact he could be borne out by many gentlemen present, who witnessed the trials at the Crystal Palace; and to assure the committee that the pumps or suction-pipes were not primed, the pump covers were removed twice in their presence. Some water was in the tank for feeding the boiler, and while the pump covers were off, one of the committee asked if the pumps could be primed from the feed-tank; the engineer in attendance showed this gentleman, by running a little water through, and this, when shown to the committee, was highly approved of as a ready way of charging the pump, if ever needed. Captain Shaw then, to avoid anything being said or thought about priming the pumps, had the tank emptied, and made the remark, "that should we require water for the boiler, he would put a few bucketsfull in the tank for us." With reference to the suction pipes being filled with water, he would state that this was impossible, as the pipes had no foot valve at the time of trial; although one was on the ground, it was decided by Captain Shaw that it should not be used. At the trial on the first day we found that our pump leaked slightly, caused by a cold

run in the metal, although it had previously stood twice the pressure. This was rectified and approved by the committee before going to work the next day. The engine then went through the heavy trial of lifting and forcing the water, and was the only engine that completed that most decisive trial, and also the severe test of throwing a vertical jet against the water tower. The author states our engine to be four or five times the power of his own, this was admitting the superiority of our engine, it was, in fact, a question of an engine of 39 cwt. against 58 cwt., or, in a weight just half as much again, we have produced an engine of 4 or 5 times the power, as was proved against the water tower, where the "Sutherland" projected a $1\frac{5}{8}$ stream, steadily to a height of 180 ft., and Mr. Roberts's engine a $\frac{7}{8}$ stream to a height of 150 ft. At the trial in Hyde Park, steam was got up in the "Deluge" to 100 lb. pressure in 11 min. 50 sec., or half the time occupied by either of Shand's engines, and pumped a large quantity of water that would have put out a considerable fire before the other engines started. This engine has the same means of cushioning as the "Torrent," and having been slightly altered since that trial, has worked very successfully at many fires and trials. To account for so many of their engines being in the Exhibition, and for the prominent position they were placed in, he would state that they offered the use of their engines gratuitously to the commissioners for service in case of fire, during the erection of the building, and for the whole time the Exhibition lasted, and he believed Messrs. Shand and Mason did the same, and hence the engines being on service, were placed in different parts of the building. The author advocates the use of fly wheels, and short strokes with high speeds; by our arrangements the length of stroke is indefinite, and always direct acting; he thought the best system was that in which the valves opened and closed the fewest number of times in a minute. With reference to the capacity of our boiler, Mr. Roberts considers 30 to 90 gallons of water a wide margin; but even when 90 gallons are in, there is more steam room left than in any other boiler; this makes it much easier and more certain work for the stoker, and if the feed pump or injector should fail, there is plenty of water in the

boiler to fall back upon, whilst getting the feed set right; with the exception of Messrs. Butt and Co.'s boiler, the "Sutherland," had boiler capacity (steam and water) of nearly twice as much as any of the boilers of engines of other makers.

The boiler capacity of Messrs. Butt and Co.'s engine was 113 gallons
,, ,, Merryweather and Son's 144 ,,

As regards the rapidity of raising steam to 100 lb. pressure, the difference between ours and Mr. Roberts's was very slight, his engine having a fly wheel started a few seconds after ours, with a rush, and having less hose to fill, had water running out at the nozzle first, ours being a direct acting engine, started a few seconds in advance, slowly and steadily, and was many minutes ahead in getting the first effective stream of water into the canvas hoods. Our steam did drop to 40 lb. pressure during the first trial, for this reason, as soon as we started the gauge glass broke, and being anxious to keep water in the boiler we kept the feed on, and with this 40 lb. pressure we were the first to fill the tank. The "Sutherland" threw a stream of water against the water tower to a height of 180 ft. for 25 minutes, and not only for a few seconds, as mentioned by the reader of the paper. One of the officials of the Crystal Palace, having at this point requested us to stop as we were inundating the cellars. He could not see how Mr. Roberts could compare the steam and water pressures of engines, without fixing pressure gauges to the steam cylinders—there were none so fixed at the trials. With reference to the "Torrent" being 40 lb. over weight, this was caused by some water being in her boiler, and as this could not be easily removed on the ground without suitable tackle for the purpose, we removed the coal bunkers and foot plates to bring it to the proper weight, but this he considered did not render it a less efficient steam fire engine; many of the American steam fire engines are without coal bunkers, as was the case with the American engine in the Exhibition.

Mr. Z. Colburn unfortunately did not hear Mr. Roberts's paper read but he had perused it, and he considered it a very carefully-prepared production. He (Mr. Colburn) had thought that some reference might have been made to Captain Ericsson's share in the application of steam to fire engines. In an early volume of the

Mechanics' Magazine reference was made to an engine as being that of Mr. Braithwaite's, but it was only a matter of right to state that it was the joint production of Captain Ericsson and Mr. Braithwaite. In New York Captain Ericsson received a prize for a design for the application of steam to fire engines when very little was known about it. This design was not, however, carried into execution, and Mr. Colburn believed that the first working engine was started in Cincinnati.

Mr. Perry F. Nursey said, it occurred to him that there was one point in connexion with steam fire engines the importance of which had not been sufficiently dwelt upon, namely, the getting up of steam quickly. He had recently taken some pains to collect information with regard to the efficiency of peat as fuel. He believed the condensed peat made under Buckland's patent would be found to possess every quality necessary to constitute a fuel adapted for steam fire and other engines. The cost did not exceed that of coal at the pit's mouth; it had been found in repeated trials to generate steam in half the time, and would do double duty as compared with coal. The absence of smoke and of clinkers were additional advantages. The only alteration necessary to its use in furnaces constructed to burn coal was that the fire bars would require to be made with lesser spaces between them,

Mr. Lee thought too much importance was attached to the fuel used in a steam fire engine, as from the amount of blast that could be obtained, either wood or the hardest coal could be used. The question of getting up steam quickly depended upon the quantity of water there was to heat; he, in the first instance, used three minute boilers, and found no difficulty with them; he had seen water thrown from an engine having one of these boilers in 3 min. 10 sec. It was found, however, that these boilers were rather sensitive, and that there was a difficulty in keeping the water at the proper height, and it was also found that these boilers were not required. He afterwards tried six minute boilers, then eight minute, and lastly twelve minute boilers, which were found to be quick enough, and were less liable to accident when left in unskilful hands. With reference to self-propelling fire engines, he had made several, and found no diffi-

culty in steering them, and could travel at any speed in the streets, faster and with greater safety than any engine drawn by horses; they could be easily turned from one street into another by one man, with perfect accuracy.

Mr. Roberts, in reply, stated, that he would endeavour to answer each speaker as briefly as possible. First, with regard to Mr. Humphreys, he begged to say that it could not be gathered from the report that the tubes were brass. Mr. Humphreys objected to the word *destructive* action, and stated it should have been *galvanic* action that took place between the iron shell, the copper pockets, and the tubes; but having lately seen a $\frac{5}{8}$ in. iron plate quite destroyed where it was in contact with copper, in less than four months, he still thought destructive a proper word. With regard to hose reels, he would simply say that Shand's was the only engine made to carry its hose upon a reel, and as it did not have the reel it could not be complete, but with regard to the steam being *bottled up in the boiler*, he did not consider Mr. Humphreys's explanation satisfactory, as they had to so urge the boiler that at times the chimney was nearly white hot and the flames nearly a yard above the top. With regard to the pressure gauge, Mr. Humphreys states that by looking at it they could see, minute by minute, that the engine was doing double the work of either of her antagonists. He (Mr. Roberts) had paid particular attention to the water pressure, having more faith in that than the quantity of water delivered into the tower. In consequence of the wind, during the whole time Shand's small engine worked in his presence, the water pressure never exceeded 70 lb. per inch with a $1\frac{1}{8}$ in. jet, and 60 to 65 with a $1\frac{1}{4}$ in. jet; this was exactly the same as the "Alexandra," and a trifle more than the "Torrent," as that kept 60 lb. with a $1\frac{1}{8}$ in. jet, and the "Princess" kept from 75 to 80 lb. with a $1\frac{1}{4}$ in. jet, and this, too, with a *half yard of canvas* under the lower bucket, while Shand and Co.'s engine was quite disabled by a *few pieces of shaving*. Mr. Humphreys had given some figures to show that several of the pumps had areas only equal to half the cylinder, and claimed as a superiority for Shand's engine that the area of their pump was only one-fifth less. He (Mr. Roberts) had no doubt that was the *secret of the bottling up of the steam;* the

areas of pumps and cylinder being so nearly equal, a very much greater pressure of steam had to be kept, nearly two to one, and in the engines having the pumps of half the capacity of cylinder, steam and water were nearly equal, and in some of the American engines, where the pumps were only about one-fourth the area of cylinders, the water pressure frequently rose above the steam; he ventured to say that had the wind been blowing when Shand's engine was tried, as it did when the others were, instead of being a dead calm, the state of the figures would have been at least reversed. Mr. Humphreys had very properly described the action of Shand and Co.'s engine as snatching at the water, when it is remembered that the pump is a single acting lift, and *snatches* up as much water with the up stroke as it delivers with up and down—in other words, a column of water (if the suction hose is 3½ in. diameter) of 53 in. high is started, and stopped at every stroke perhaps 150 to 200 times a minute, he thought it would be seen that it carried an element of destruction that must very quickly bring it to grief. With regard to his (Mr. Roberts's) pump being like Shand's: first, a glance at the drawings would show that there was no similarity whatever, as in Shand's the buckets were not placed over each other, and the angle made by the connecting rods was from 40 to 45 deg., and in the author's pump the buckets were placed quite over each other, and the angle made by the slings never more than 2 deg. One word more: a glance at the drawings would show that his (Mr. Roberts's) engine was as direct acting as Messrs. Shand's, the only use of the shaft in his being to work the slide, while Messrs. Shand's had to work their feed pump in addition. With regard to Mr. Olrick's remarks, he would not attempt to follow him through all of them, but there was no doubt if the wind had not blown at all during the trials, but had been as quiet as it was when Messrs. Shand's small engine was tried, that the trials would have been of infinitely more value than they were; but the wind did blow, and that with a continually varying force. To show the effect of the wind he might state, that Shand's large engine threw in the first hour into the tower 20½ in., his (Mr. Roberts's) engine the same exactly; the second hour the wind did not blow so much for Shand, and the engines delivered

25 in., and the reverse having taken place during his (Mr. Roberts's) second hour he only delivered $15\frac{3}{4}$ in. With regard to the injector, perhaps Mr. Olrick had not much experience in the working of it. He (Mr. Roberts) had two at work for months, sometimes they would go well for weeks and then fail, and for hours give trouble, and then go on again; he had tried several others, and must say that for small boilers and very high pressure he would not trust them, but always fit pumps. With regard to the complication of gear to drive the slide rods, Mr. Olrick stated that there were only four pieces to drive the slide valve in the "Torrent," and nine in his (Mr. Roberts's). The author, by referring to the diagrams, showed there were nine pieces in his engine, including the slide valve, and no less than *fourteen* in the "Torrent" instead of *four* only, besides this the small valve i^2, fig. 2, is started with a jerk, when the adjustable stops upon the disc s come in contact with the tongued lever t, thus, as before stated, going back to the tappet motion; besides this there are the extra ports and cocks for cushioning, without which this class of engine would not work an hour, but which is all unknown in the author's engine. There were only two other remarks of Mr. Olrick's he would notice: first, Mr. Olrick stated the "Sutherland" was the only engine that lifted the water the 18 feet lift; this was quite incorrect, as several others did it, among them the author's engine, although it had the piece of canvas in it; the other, that the pumps should have long strokes and moderate velocity, but how did his pet engine answer in these points? The stroke of the "Sutherland's" pumps was 2 ft., this, with 80 double strokes per minute, gives a speed of 320 ft. per minute, and his pump $3\frac{1}{8}$ inch stroke with 200 strokes, gives a speed 104 ft. per minute, or about $\frac{1}{3}$ the speed of the "Sutherland." This would also apply to the remark of Mr. Young, who said he was convinced that a slow, steady, reciprocating motion is the right thing (and leaving out the next sentence, he says), the slow speed of the pumps is important, as it gives a full pump, and is especially valuable in using (as frequently happens) gritty or sandy water, and *the higher the speed* at which the engine is run the more necessary *does the slow motion become.* All this he (Mr. Roberts) fully agreed with, and, as in all the engines tried, the speed of pistons and the speed of pump

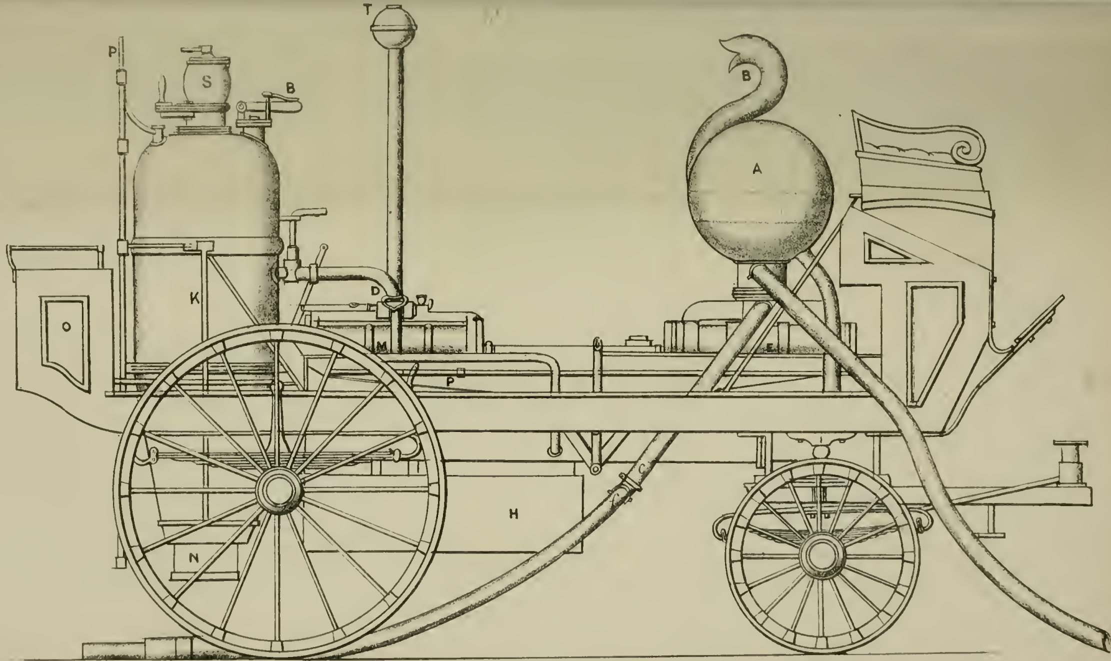
plungers was the same, except the author's engine, in which the stroke of pumps bucket is not quite one-fourth of the piston, he must conclude that Mr. Young was alluding to his engine; although he does not approve of crank or fly-wheel, he thanked Mr. Young for giving the correct time of getting steam by his (Roberts's), 10 min.; Merryweather, 10 min. 15 sec.; Shand, 11 min. 45 sec.; Easton and Amoss, 16 min.; and Lee, 17 min. With regard to Mr. Field's boiler, he had no hesitation in saying he considered it a first-rate boiler, but was afraid a careless stoker might break off some of the tubes, and he must say he preferred his own, because it got steam quicker and kept it more steadily, there appeared a more perfect combustion, and there was no fear of the tubes being injured, besides which it had perfect circulation and no priming. With respect to Mr. Lee's remarks upon self-propellers, he had no hesitation in saying he could go quite as safely at 18 or 20 miles anhour as horses could be driven at 12.

The Chairman said that he thought Mr. Roberts deserved the thanks of the meeting for a very carefully prepared paper, which had been listened to with attention; he thought that care had been taken with the paper, and likewise with discussion, to avoid personalities; it had been hinted that the class of paper was objectionable on account of the inducement offered to indulge in deprecation of one manufacturer's machine as compared with another's, thereby leading to personalities, so desirable to be avoided—this class of paper certainly allowed opportunities for this, as had been too distinctly shown during some parts of the discussion, but still the Chairman was of opinion the good such papers did to the society, and their importance generally to the profession, much counterbalanced any evil resulting from the ebullition of feeling on the parts of those interested in the different machines which the society had under discussion, and he felt assured the good practical sense of the members would always avoid extremes and disagreeable personalities in discussion on this class of paper. As far as the paper and discussion went, it certainly tended to show the great difficulty the committee at the Crystal Palace had to contend with in determining the most efficient machine; but there was nothing brought before the society that tended to

upset in any way the judgment of the committee: that the committee were gentlemen, who would give a most fair and impartial judgment, he thought no member of the society for a moment could doubt. Some members had asserted that the American engine had not been fairly dealt with, in fact, that the accident to the "Manhattan" was intentional; he would not allow that any body of Englishmen, more particularly those connected with the engineering profession, would be guilty of such highly disreputable, and reprehensible conduct, he believed such statements utterly groundless. In conclusion, the Chairman stated that he hoped this discussion would lead to good results, in the profession turning their attention to making a more perfect steam fire engine than had yet been produced.

THE END.

C. WHITING, BEAUFORT HOUSE, STRAND.

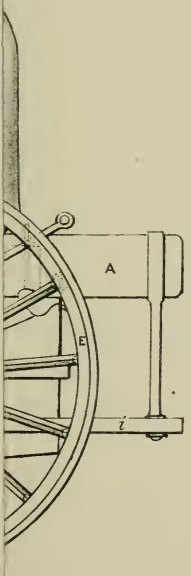


BRAITHWAITE & ERICSSON'S STEAM FIRE ENGINE. 1829.

E. AND F. N. SPON BUCKLERSBURY LONDON

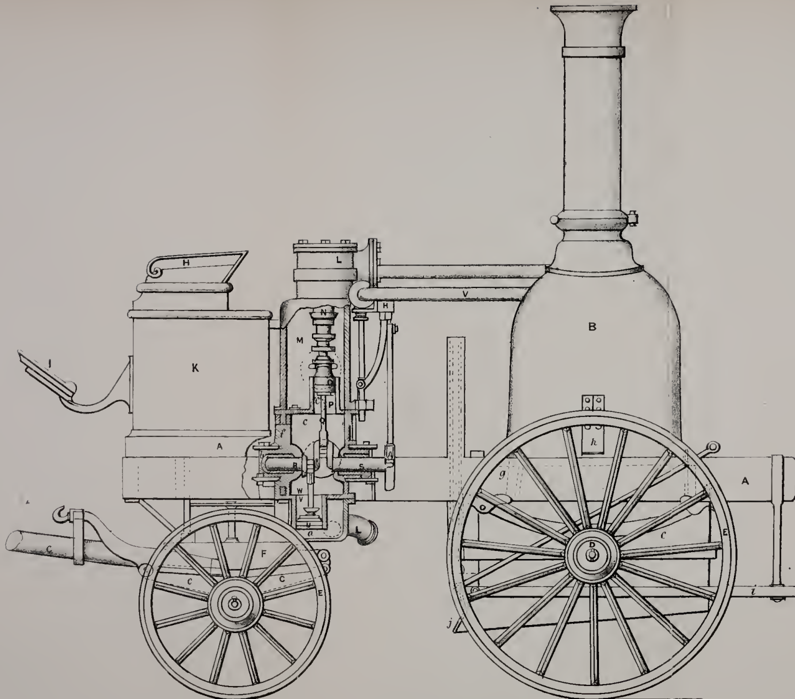
Upfield Green Lith.

No. 2.



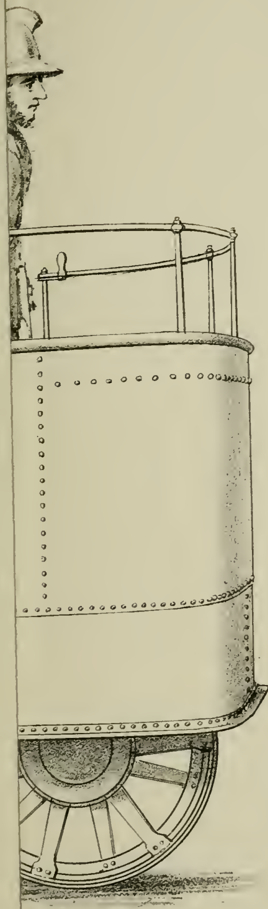
1858.

Upfield Green, Lith.

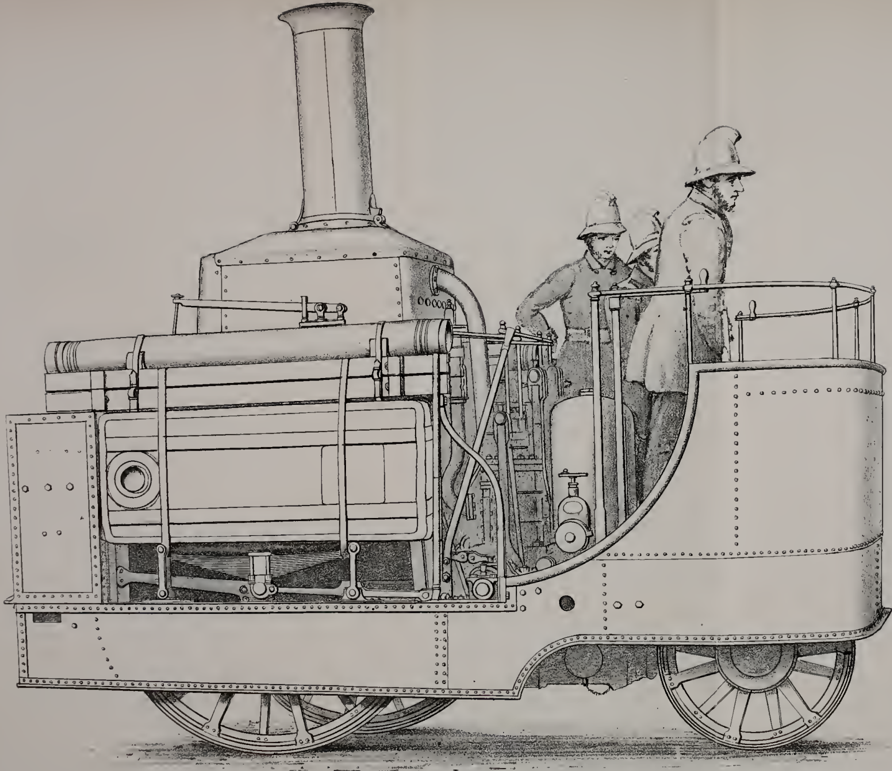


MESSrs SHAND & MASON'S STEAM FIRE ENGINE, 1858.

E AND F N SPON, BUCKLERSBURY, LONDON

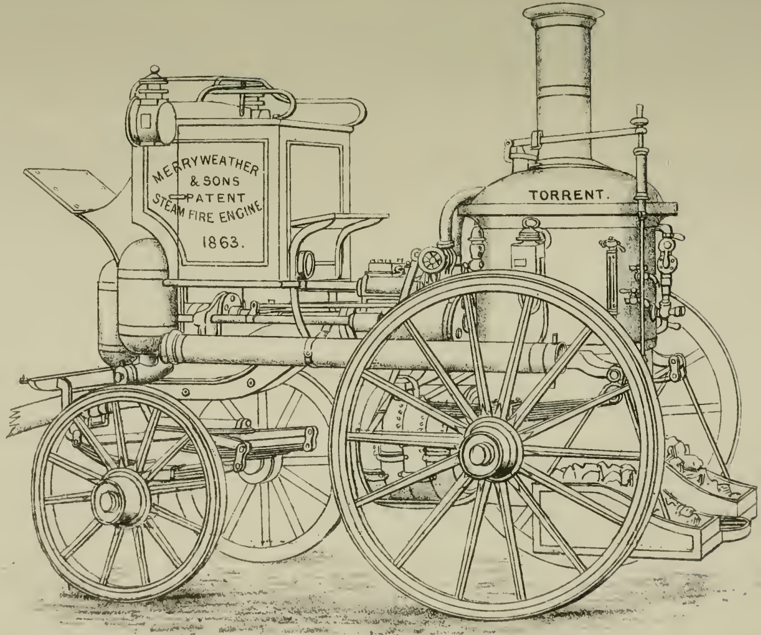


&c.



W. ROBERTS' SELF-PROPELLING STEAM FIRE ENGINE, HOIST, &c.
IN RUNNING ORDER.

E. AND F. N. SPON BUCKLERSBURY LONDON



MESSrs. MERRYWEATHER, SONS & FIELD'S LIGHT STEAM FIRE ENGINE "TORRENT".

No. 5.

FIG: 1.

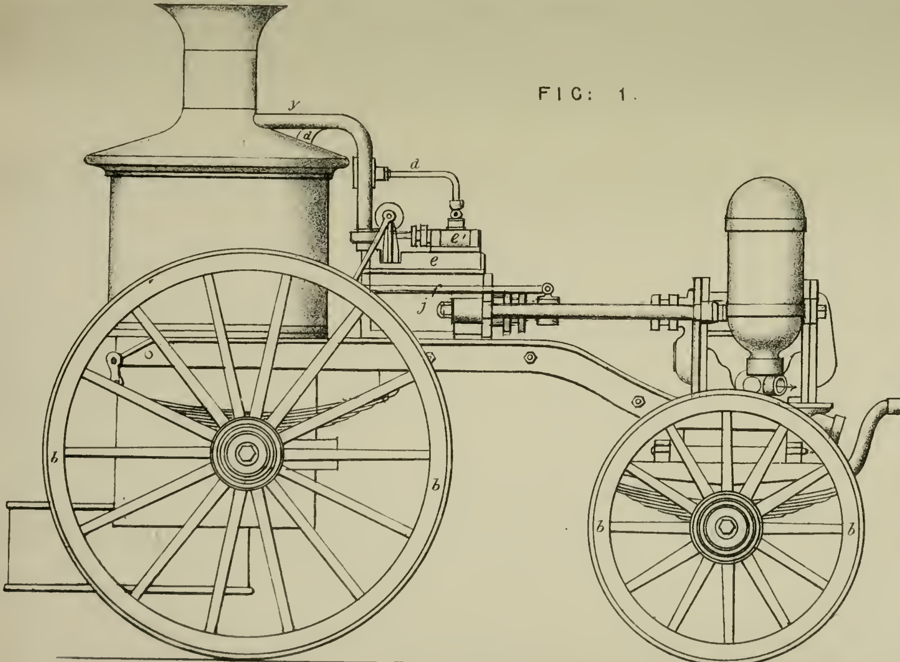
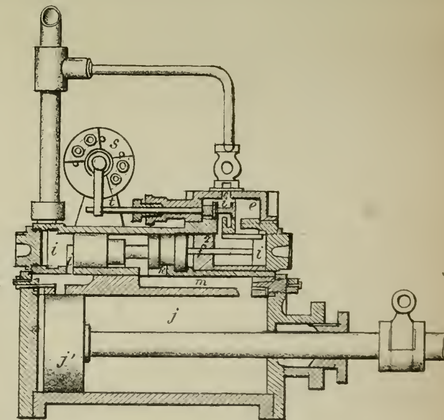


FIG: 2.



SECTION OF CYLINDER.

MESSrs MERRYWEATHER, SONS & FIELD'S PATENT STEAM FIRE ENGINE "TORRENT".

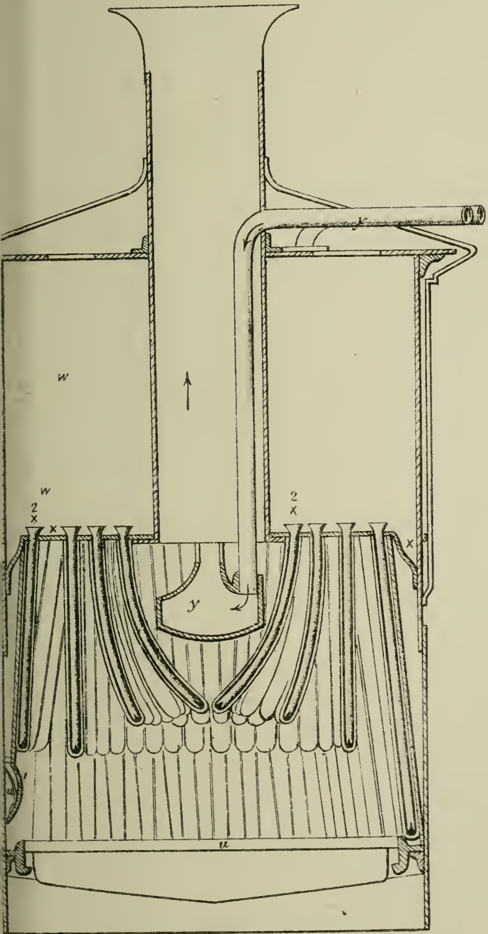
E AND F N SPON, BUCKLERSBURY, LONDON

Enfield Green, Lith.

No. 6

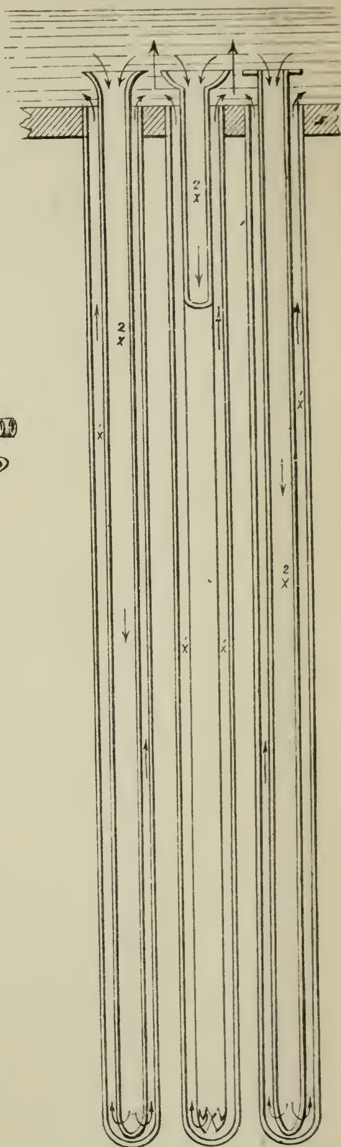
MESSrs. MERRYWEATHER, SONS & FIELD'S BOILER.

FIG: 3.



SECTION OF BOILER.

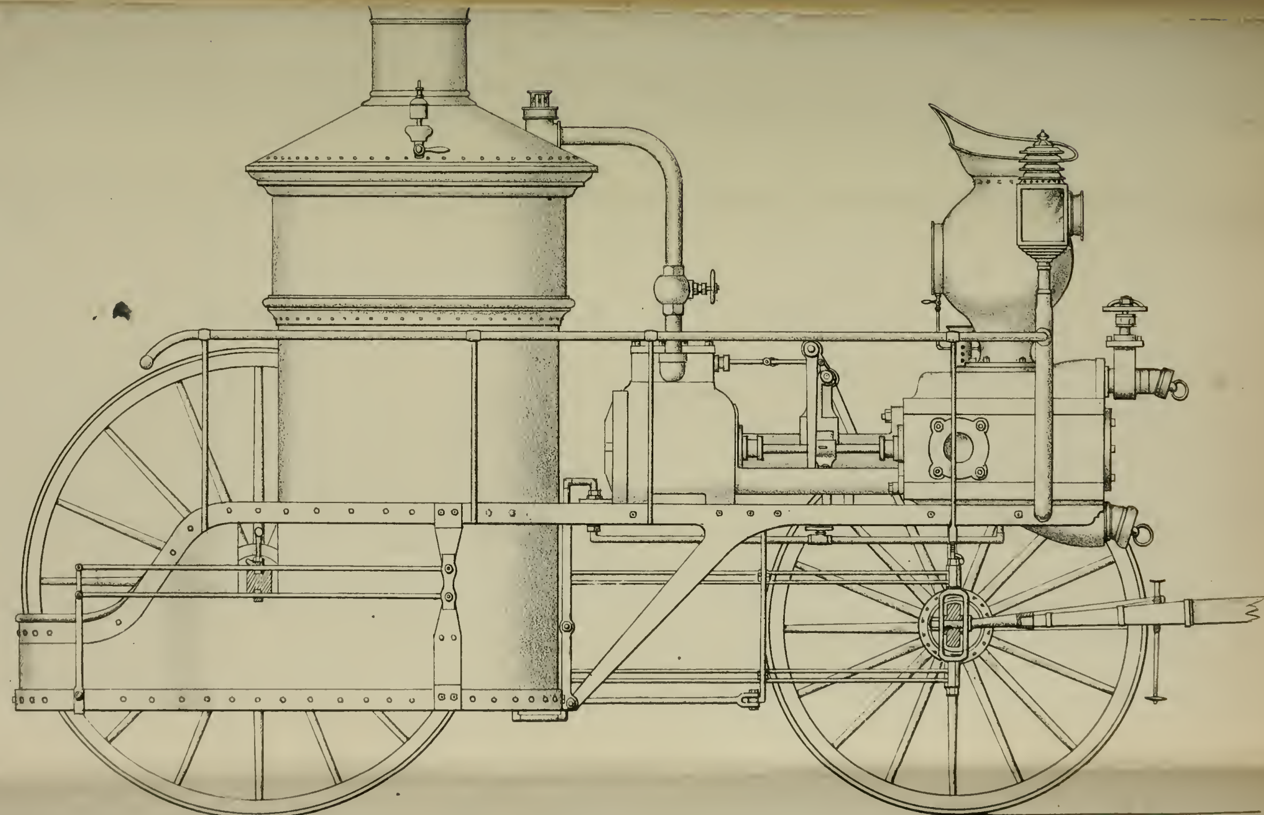
FIG. 4.



ENLARGED VIEW OF BOILER.

E. AND F. N. SPON, BUCKLERSBURY, LONDON

Upfield Green, Litho.

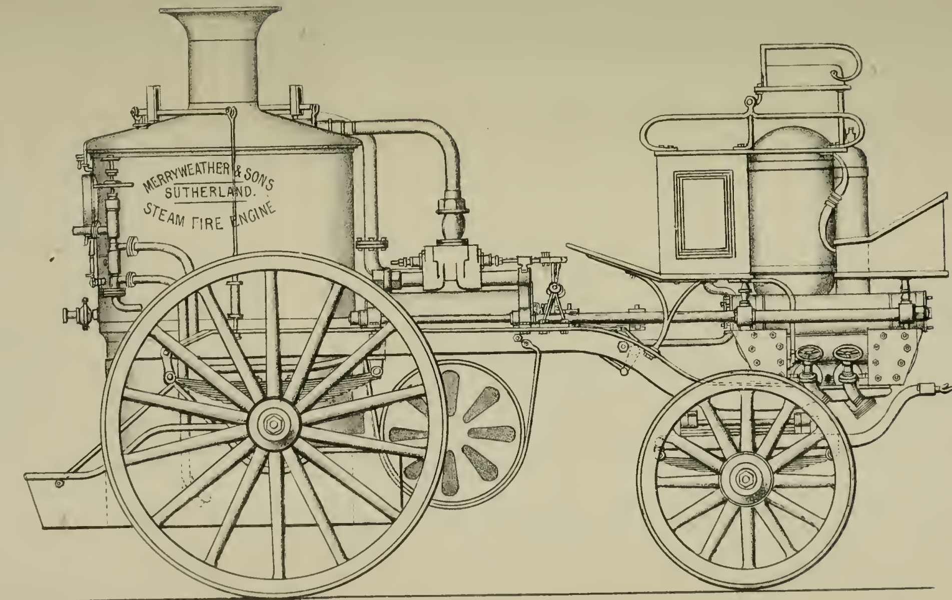


MESS^{rs} LEE & C^{o}'s STEAM FIRE ENGINE — "ANNIHILATOR."

(Built by Mess^{rs} Easton Amos & Sons.)

E AND F N. SPON, BUCKLERSBURY, LONDON

Sifield Green, Lith



MESSrs. MERRYWEATHER & SONS' STEAM FIRE ENGINE "SUTHERLAND".

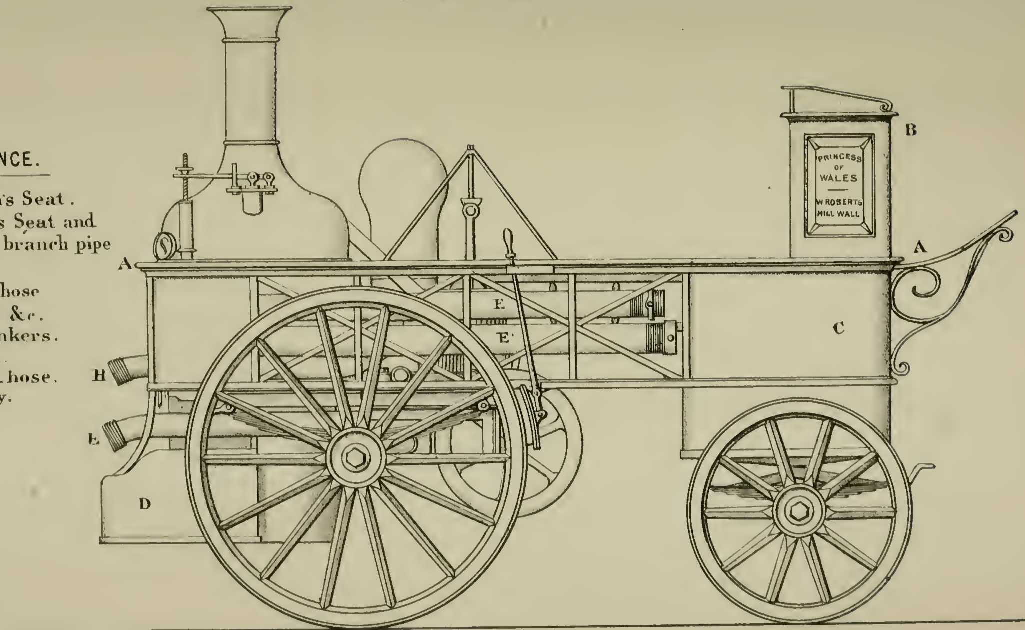
Enfield Green Lith.

W. ROBERTS' PATENT STEAM FIRE ENGINE.

SIDE ELEVATION.

REFERENCE.

A A. Firemen's Seat.
B.__ Driver's Seat and box for branch pipe &c.
C.__ Box for hose buckets &c.
D __ Coal bunkers.
E __ Suction.
E' __ Suction hose.
H __ Delivery.

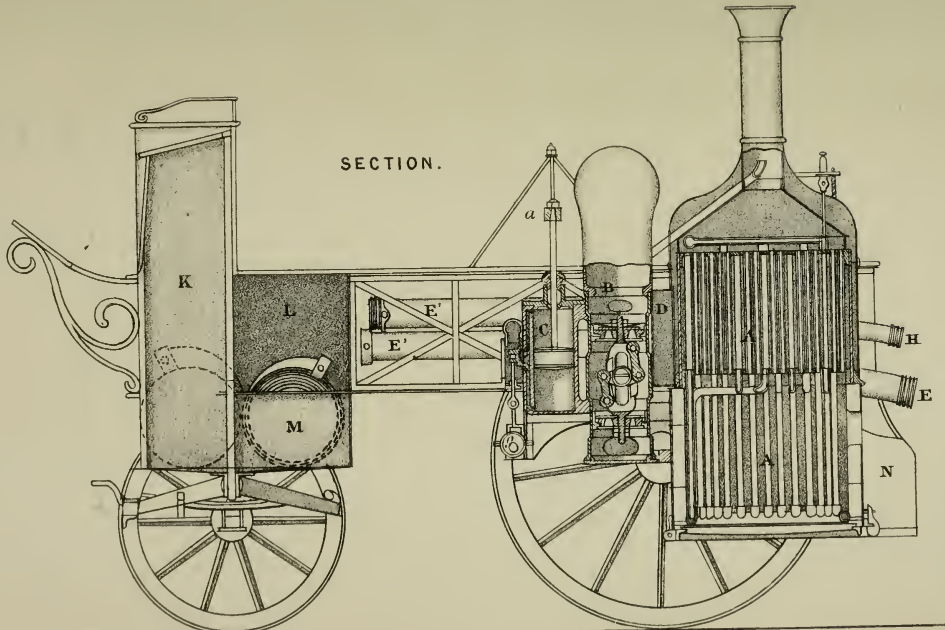


E AND F N. SPON, BUCKLERSBURY, LONDON

Ipfield Green, Lit

W. ROBERTS' STEAM FIRE ENGINE "PRINCESS OF WALES".

SECTION.



REFERENCE.

A A. Boiler.
B. Pump.
C. Engine.
D. Water tank.
E. Suction pipe.
E'E' d°. hose.
H. Delivery pipe.
K. Box for branches &c.
L. d°. .. delivery hose. Gear &c.
M. Engineer's tool box.
N. Coal bunker.

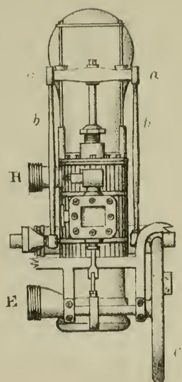
E. AND F. N. SPON, BUCKLERSBURY, LONDON

Upfield Green, Lith.

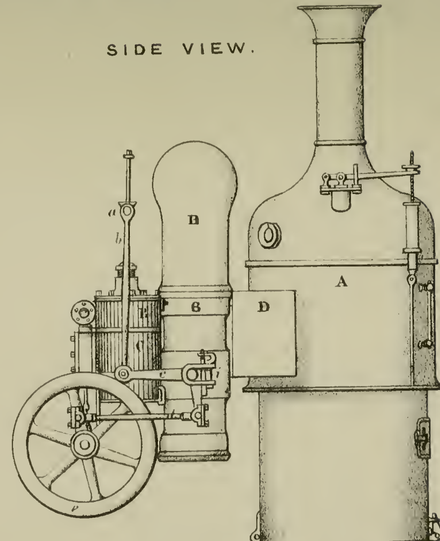
W. ROBERTS' PATENT STEAM FIRE ENGINE.

ENGINE & PUMP.

END VIEW.



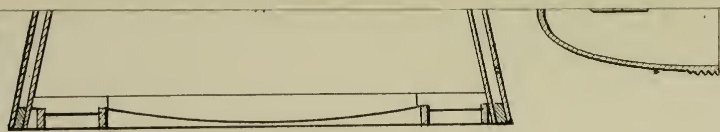
SIDE VIEW.



REFERENCE.

A Boiler.
B Pump.
C Steam Cylinder.
D Water Tank.
E Suction Pipe.
H Delivery Pipe.

No. 12

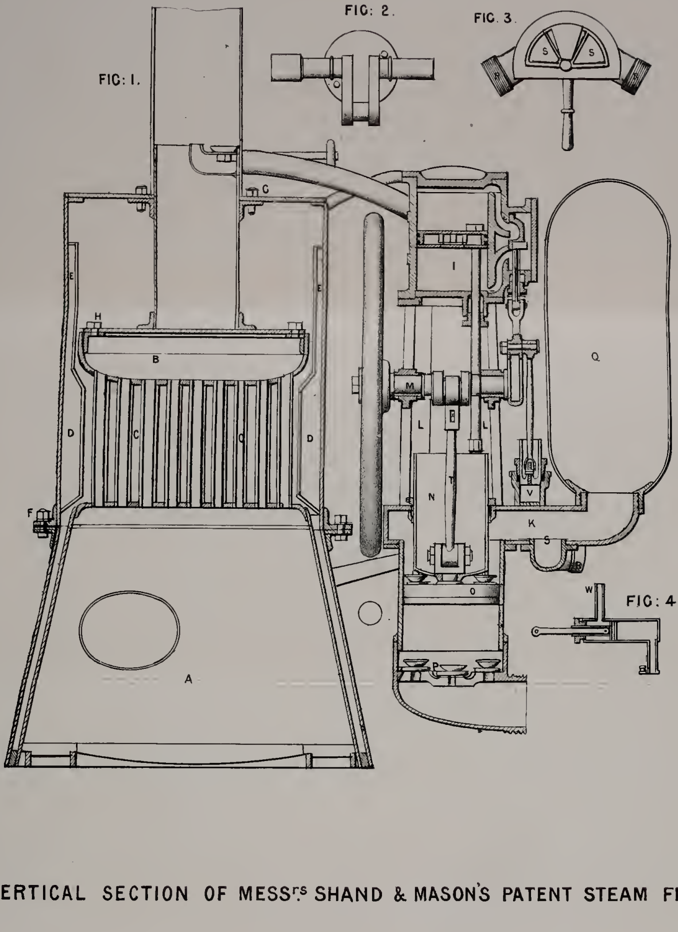


VERTICAL SECTION OF MESSrs. SHAND & MASON'S PATENT STEAM FIRE ENGINE.

E. AND F. N. SPON, BUCKLERSBURY, LONDON

Upfield Green, Lith.

FIG: 1. FIG: 2. FIG. 3. FIG: 4.



VERTICAL SECTION OF MESSrs SHAND & MASON'S PATENT STEAM FIRE ENGINE.

E AND F N. SPON, BUCKLERSBURY, LONDON

Upfield Green, Lith.

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